

EXPERIMENTS
AND
OBSERVATIONS,
RELATING TO VARIOUS BRANCHES OF
NATURAL PHILOSOPHY;

WITH
A CONTINUATION
OF THE
OBSERVATIONS ON AIR.

VOL. III.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

AC. IMP. PETROP. R. PARIS. HOLM. TAURIN. AUREL. MED.
PARIS. CANTAB. AMERIC. ET PHILAD. SOCIUS.

....Trahit quodcunque potest, atque addit acervo.

HORACE.

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TO
WILLIAM CONSTABLE, Esq;

OF
BURTON CONSTABLE, IN YORKSHIRE,

THIS VOLUME

IS,

WITH THE GREATEST RESPECT
AND GRATITUDE,

INSCRIBED,

BY HIS OBLIGED,

HUMBLE SERVANT,

J. PRIESTLEY.

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THE

P R E F A C E.

THIS volume, like each of the five preceding ones, contains an account of many *new facts*, with several illustrations of those discovered before; but withal many *new difficulties*, such as must excite a desire of making farther progress in these investigations: so far are we from seeing any end of our labours.

This, however, is a consideration by no means discouraging to the true philosopher. On the contrary, it is calculated to open to him the most pleasing of all prospects, as it shows us that there will never be wanting proper objects of these investigations, than which nothing can furnish more delightful employment to the mind of man, and which

promise advantages that will abundantly compensate for all the labour that we can bestow upon them.

As we ourselves are parts of the great system of nature; as the laws of nature comprehend, and continually affect us; and every thing that we do is putting things into situations, in which the laws of nature determine the result; the more perfect knowledge we have of those laws, the better we must be able to foretel those results, and therefore to chuse what we wish to produce. Consequently, the more knowledge we have of the laws of nature, the greater power we shall have over nature, and the more commodious we shall be able to make our situation in the world, while it remains constituted as it is. And as we are at present in our infancy with respect to the knowledge of nature, we may assure ourselves, we are likewise but in our infancy with respect to the power we may acquire of guarding ourselves against the inconveniencies, and securing the advantages of our situation.

It is nothing but a superior knowledge of the laws of nature, that gives Europeans the advantage they have over the Hottentots, or the lowest of our species. Had these people never known Europeans, they could not have formed an idea of any mode of life superior to their own, though it differs but little from that of the brutes. In like manner, science advancing, as it does, with an accelerated progress, it may be taken for granted, that mankind some centuries hence will be as much superior to us in knowledge, and improvements in the arts of life, as we now are to the Hottentots, though we cannot have any conception what that knowledge, or what those improvements, will be. It is enough for us to see that nature is inexhaustible, that it is a rich mine, in which we shall never dig in vain, and that it is open to infinitely more labourers than are now employed in exploring its contents, or in digging for them.

Having been a pretty successful adventurer in this great mine, my philosophical friends

in general wonder that I do not confine my attention to it. Their dissatisfaction with me is so great, and I hear of it from so many quarters, that I think it right to take some opportunity (and a better than the present will hardly occur) to make an apology for my conduct, especially to those of my friends, by whose assistance I am enabled to give my time to these liberal pursuits; being pleased to think that my attention to them will be of some advantage to science and the public.

In the first place, I would observe, that I follow my own best judgment in devoting my time to what I really apprehend to be the most important pursuits, those from which myself, and mankind at large, will finally derive the greatest advantage; and I must be allowed to say, that the greater variety of objects to which it is evident that I have given attention, must qualify me to be a better judge in this case than those who censure my conduct. Persons who have only one object of pursuit, never fail to over-rate it, and of course to undervalue other things.

I would

I would farther observe, that the attention I have given to *theology* (which, by the way, is my original and proper province, and for which I may, therefore, be allowed to have a justifiable predilection) does not engross so much of my time as some persons may imagine. I am particularly complained of at present, as having thrown away so much time on the composition of my *History of the Corruptions of Christianity*, and of the *Opinions concerning Christ*. But I can assure them, and the nature of the thing, if they consider it, may satisfy them, that the time I must necessarily have bestowed upon the experiments of which an account is contained in this single volume, is much more than I have given to the *six*, of which the above-mentioned works consist, and to all the controversial pieces that I have written in defence of the former of them. The labour and attention necessary to enable me to write single paragraphs in this work, have been more than was requisite to compose whole sections, or chapters, of the former. In general, during the composition of those works,

works, the greatest part of every day was spent in my laboratory, and the evenings and mornings only in reading or writing. Besides, these different studies so relieve one another, that I believe I do more in each of them, by applying to them alternately, than I should do, if I gave my whole attention to one of them only.

But my principal defence rests on the superior dignity and importance of *theological studies* to any other whatever. Every rational being ought to distinguish, by the greater attention that he gives to them, those objects which are of the greatest importance to himself, and to mankind at large. And certainly, if there be any just rule for estimating the value of a problem, or query, that is proposed to us, we must think it of infinitely more moment to discover whether there be a future, and especially an endless life after this, and how to secure a happy lot in that future life, than to make the best provision possible for ourselves in this life, which is the ultimate object of all natural philosophy.

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philosophy. Studies, but remotely connected with that great object, must have a dignity and importance infinitely superior to any other. A man must never have thought a moment on the subject, if he hesitate to give a decided preference in the case. To think or act otherwise, would be like a man busying himself about farthings, who has large estates, or kingdoms, depending, and who should neglect the latter in order to secure the former.

All that any philosophical person can pretend to say in the case, must be, that the expectation of a future life is so manifestly chimerical, that it can never be worth a wise man's while to lose a moment in thinking about it, or to employ his time in any study relating to it. This I know to be the opinion of many who will read this book, if not this preface. But in this I must take the liberty to differ from them, and for reasons which I shall submit to their serious consideration.

Natural

Natural phenomena, I agree with them, are unfavourable to any expectation of a future life, and the doctrine of an *immaterial soul*, capable of subsisting and acting when the body is in the grave, on which the doctrine of a future state is generally founded, I am as fully persuaded as they can be, is unauthorized by any natural appearances whatever. My expectation of a future life rests on another foundation; and, improbable as I acknowledge the doctrine to be, according to the light of nature, it is nevertheless such as I firmly believe, on the plainest of all evidence; the author of nature having given us an absolute assurance of it, by persons authorized to speak in his name, and whose divine mission was proved by such works as no other than the author of nature could have enabled them to perform.

That such works have been performed, and for this important purpose, must, I apprehend, be true, if there be any truth in history. And there is no kind of evidence
more

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more easily subjected to a rigorous examination than that which is of the historical kind, the maxims of which we are every day conversant with.

Now it appears to me, that we must either admit the truth of the gospel history, which contains an account of the doctrine, miracles, death, and resurrection of Christ (on which the belief of a future life depends) or believe what is infinitely more incredible, viz. that several thousand people, present at the transactions, and who had no motive to believe them without sufficient evidence, but every motive to turn their eyes from them, or disbelieve them if they could, should yet, without such evidence, have given the firmest assent to them, and have entertained so little doubt of the extraordinary facts, as to maintain their faith in them at the hazard of every thing dear to them in life, and even cheerfully lay down their lives, rather than abandon their faith. Let philosophers, as such, account for this great *fact*, without admitting more real miracles, and those of a
more

more extraordinary kind, than the belief of christianity requires of me, and I will relinquish my present faith, dear as it is to me, and join them in exposing it.

As philosophers, the question between us is, whose faith, strictly speaking, is more agreeable to *present appearances*. Whatever we may think of an *author of nature*, and of his attention to it, we equally believe in the *uniformity of the laws of nature*, and that man, whose constitution is a part of the system of nature, was the same kind of being two thousand years ago that he is now; as much as that a horse of that age, or an oak tree of that age, had the same properties with the horses and oaks of the present. Consequently, whatever was possible with respect to man in any former period, is equally possible now.

But will any man, who gives a moment's attention to the subject, say that it is even *possible* that several thousand persons, in London or Paris, could be made to believe that

that any man in London or Paris, died and rose from the dead in their own life-time, that they should persist in this persuasion through life, without shewing any sign of insanity, that they should gain numerous proselytes to their opinion, though it subjected all who embraced it to all kinds of persecution, and even to death; and that the belief of it should establish itself against all opposition, without any person being able to detect the imposition?

Now I apprehend that this might take place more easily in London, or in Paris, at this day, than it could have done at Jerusalem in the time of our Saviour. Human nature could not have been the same thing then that we find it to be at present, if mankind could have been so imposed upon. This I therefore think absolutely incredible, and consequently, as the less difficulty of the two, as believing a thing much less improbable, I admit the truth of the gospel history, the admission of which makes the subsequent account of the propagation of christianity (which

(which all history, and even the present state of things proves to be true) perfectly easy and natural. Admitting these leading facts, all the rest follows of course, and all things came to be as they are without any farther miracle. But real miracles we must have somewhere, in order to account for the present state of things; and if we must admit miracles, let them be such as have a *great object*, and not such as have no object at all, but only serve to puzzle and confound us.

The history of the Jews, and the books of the Old Testament, furnish many *facts*, which no hypothesis besides that of the divine origin of their religion can explain. Let the philosopher only admit as a *postulatum* that Jews are, and always were, *men*, constituted as other men are, and let him not deceive himself, by considering them as beings of another species. All I wish in this respect is, that persons who pretend to the character of *philosophers*, would be so throughout, and carry the same spirit into the study of history, and of human nature,

(which)

that they do into their laboratories; first assuring themselves, with respect to *facts*, and then explaining those facts by reducing them to *general principles* (which, from the uniformity of nature, must be universally true) and then I shall have no doubt of their becoming as firm believers in christianity as myself. They will find no other hypothesis, that can explain such appearances as they cannot deny to be real. Let philosophers now say, whether there be reason in this, or not.

I therefore take the liberty, having been led to advance thus much, to address my brother philosophers on a subject equally interesting to us as *philosophers*, and as *men*. Do not disregard a question of infinite moment. Give it that degree of attention to which it is naturally intitled; and especially do not so far abandon the serious character of *philosophers*, as to *laugh* where you ought to *reason*. At least, do this great subject, and yourselves, the justice

to consider the *facts*, and endeavour to frame some *hypothesis* by which to account for them; and do not decide in half an hour, on an inquiry which well deserves the study of a great part of your lives.

If I have a stronger bias than many other persons in favour of christianity, it is that which philosophy gives me. I view with rapture the glorious face of nature, and I admire its wonderful constitution, the laws of which are daily unfolding themselves to our view. It is but little that the life of man permits us to see at present, and therefore I feel a most eager desire to renew my acquaintance with it hereafter, and to resume those inquiries with which I am so much delighted now, and which must be interrupted by death.

Could I imagine that the knowledge of nature would ever be exhausted, and that we were approaching to a termination of our inquiries, I could more contentedly shut my eyes

eyes on a scene in which nothing more was to be seen, or done. But to quit the stage at present (and I believe the aspect of things will be exactly similar in any future period of our existence) without the hope of re-visiting it, would fill me with the deepest regret. The general who, like Epaminondas, or Wolfe, dies in the arms of victory, dies with satisfaction; but not so he that is cut off in the beginning of a doubtful, though promising engagement. Thus I feel on the idea of ceasing to breathe when I have but just begun to know what it is that I breathe.

M. HERSHEY'S late discoveries in, and beyond, the bounds of the solar system, the great views that he has given us of the arrangement of the stars, their revolutions, and those of the immense systems into which they are formed, are peculiarly calculated to inspire an ardent desire of seeing so great a scene a little more unfolded. Such discoveries as these, give us a higher idea of the value of our being, by raising our ideas of the

system of which we are a part, and, with this, an earnest wish for the continuance of it.

Besides, *civil society* is but in its infancy, the world itself is but very imperfectly known to the civilized inhabitants of it, and we are but little acquainted with the real value of those few of its productions of which we have some knowledge, and which we are only beginning to name, and to arrange. How must a *citizen of the world* wish to know the future progress of it.

To have no wish of this kind certainly argues a low, an ignoble, and I will say, an unphilosophical mind. I consider all such persons, how superior soever they may be to myself in other respects, with pity and concern. They would have unspeakably more satisfaction in their philosophical pursuits, if they carried them on with the views of things that I have. It has been justly observed, that great views indicate, and indeed constitute, great minds. What elevation of
mind,

mind, then, would the prospects of the christian, add to those of the philosopher*.

With men of reflection this apology for my conduct will, I doubt not, be admitted as satisfactory; and till I hear better reasons than have yet been offered to me for changing my conduct, I shall continue to give my attention to my different pursuits, according to my own ideas of their respective im-

* If any of my philosophical friends should be induced, by what I have here urged, to look into my *theological writings*, I would take the liberty to recommend to them my *Letters to a Philosophical Unbeliever*, the *Institutes of natural and revealed Religion*, and the *History of the Corruptions of Christianity*, especially the *Conclusion*, part I. relating to Mr. GIBBON, who has declined engaging in the discussion I there proposed to him. If they wish to see more particularly in what manner christianity came to be encumbered with the doctrine of *the trinity*, which has been the foundation of one of the greatest objections to it, I would further refer them to my *History of early opinions concerning Christ*, where they will see it traced to its proper source in the Platonic philosophy, and where it is proved that the primitive christian church was unquestionably unitarian.

portance; and my friends have no reason to fear that I shall neglect *philosophy*. It has, perhaps, but too strong charms for me. I shall endeavour, however, to keep it in its proper place, and not so much attach myself to the study of the laws which govern *this* world, as to lose sight of the subserviency of this world, and of all things in it, to *another* and a better; in which I hope to resume these pleasing philosophical pursuits, and to see, in a comprehensive view, those detached discoveries which we are now making here.

At present all our *systems* are in a remarkable manner unhinged by the discovery of a multiplicity of *facts*, to which it appears difficult, or impossible, to adjust them. We need not, however, give ourselves much concern on this account. For when a sufficient number of new facts shall be discovered (towards which even imperfect hypotheses will contribute) a more *general theory* will soon present itself; and perhaps to the

the most incurious and least sagacious eye. Thus, when able navigators have, with great labour and judgment, steered towards an undiscovered country, a common sailor, placed at the mast head, may happen to get the first sight of the land. Let us not, however, contend about *merit*, but let us all be intent on forwarding the *common enterprize*, and equally enjoy any progress we may make towards succeeding in it; and above all, let us acknowledge the guidance of that Great Being, *who has put a spirit in man, and whose inspiration giveth him understanding.*

In this volume I have continued to express the degree of purity in air as in the former; saying, for instance, that air is of the standard of 1.2 or 1.4, when one measure of the air to be examined, and one of nitrous air, presently after being mixed together, occupy the space of one measure, and two, or four-

tenths of a measure. But if the air be called *dephlogisticated*, it is always to be understood that *two* equal measures of nitrous air were used. The *weights* mentioned in this volume are *ounces* and *grains*, the ounces being English troy weight, containing 480 grains. I have dropped the use of the *penny-weight* (*dwt.*) which is twenty-four grains, ~~or one sixteenth of a pound troy~~, in consequence of finding that it occasioned some difficulty to foreigners.

*An explanation of the Figures in the plate prefixed to
this volume.*

Fig. I. is a view of the apparatus with which the principal experiments relating to the seeming conversion of water into air were made. It consists of an *earthen vessel*, the bulb of which, containing moistened clay, is fixed in the inside of a *glass vessel*, through which the heat of a burning lens may be thrown upon it; while the inside has a communication with a *basin* of water, or mercury, in which vessels may be placed to receive the air that is forced through the body of the earthen vessel; while the water, or mercury, in the *basin* in which the glass vessel stands, rises within it, to supply the place of that air.

Fig. II. Shews the disposition of the apparatus by which steam is transmitted through a red-hot tube, containing iron, &c. with a *worm tub* to collect the superfluous water, &c. and a vessel to receive the air that is produced. This vessel is here drawn very small, that it might not take up much room in the plate; but I have generally used a large trough for this purpose, and jars of considerable size to receive the air. Instead of
the

the small *furnace* to heat the water, &c. I now use one of *Mr. Argand's lamps*, which is, on several accounts a very valuable addition to a chemical apparatus. Fig. VI. represents the method of receiving the air in this process under a funnel, fixed in a trough of water, which may be used when large balloons are filled, and when no account is taken of any water that is condensed in the process.

Fig. III. represents the contrivance, mentioned page 135, by means of which all the air that was driven out of a vessel, by pouring mercury into it, was obliged to pass through a thin glass vessel, in which it might deposit the moisture, &c. it contained.

Fig. IV. represents a large glass balloon, in which inflammable air, issuing from the orifice of a small tube, burns like a candle, while the water produced by the process is collected in the inside of it.

Fig. V. represents a strong cylindrical glass vessel, in which inflammable and dephlogisticated air may be fired. It is furnished with a wooden cap, firmly cemented to the open end of it, and closed with a screw, and two iron wires are inserted at the top of it, between which an electric spark can be taken.

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ERRATA, &c.

N. B. (b) signifies from the bottom of the page.

Page 152. l. 8. *for seventeen, read sixteen.*

— l. 10. (b) *for three times, read between four and five times.*

— 232. l. 9. *for woad, read wad.*

— 291. l. 4. *for water, read of water.*

— 303. l. 8. (b) *dele yet.*

— 319. l. 10. (b) *for crucible, read retort.*

— 328. l. 1. (b) *add as may be inferred from part of it being converted into
pure air by the electric spark.*

— 354. l. 9. *read manganese, which I had from the glass house.*

— 375. l. 11. *read of the greatest part of its own phlogiston.*

ERRATUM.

Vol. II. p. 74. l. 4. (b) *for one ounce measure, read one third.*

OBSERVATIONS

RELATING TO

VARIOUS BRANCHES

NATURAL PHILOSOPHY.

SECTION I.

Experiments relating to Phlogiston.*

THERE are few subjects, perhaps none, that have occasioned more perplexity to chemists, than that of *phlogiston*, or, as it is sometimes called, *the principle of inflammability*. It was the great discovery of STAHL, that this principle, whatever it be, is transferable from one substance to another, how different soever in their other properties, such as sulphur, wood, and all the metals, and therefore is the same thing in them all. But

* This section is copied from the Philosophical Transactions, vol. lxxiii. p. 399, with the addition of some notes.

what has given an air of mystery to this subject, has been that it was imagined, that this principle, or substance, could not be exhibited except in combination with other substances, and could not be made to assume separately either a fluid or solid form. It was also asserted by some, that phlogiston was so far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they chose to call it *the principle of levity*. This opinion had great patrons.

Of late it has been the opinion of many celebrated chemists, Mr. LAVOISIER among others, that the whole doctrine of phlogiston had been founded on mistake, and that in all cases in which it was thought that bodies parted with the principle of phlogiston, they in fact lost nothing, but on the contrary acquired something; and in most cases an addition of some kind of air; that a *metal*, for instance, was not a combination of two things, *viz.* an *earth* and *phlogiston*, but was probably a simple substance in its metallic state;

state; and that the calx is produced not by the loss of phlogiston, or of any thing else, but by the acquisition of air.

The arguments in favour of this opinion, especially those which are drawn from the experiments that Mr. LAVOISIER made on mercury, are so specious, that I own I was myself much inclined to adopt it. My friend Mr. KIRWAN, indeed, always held that phlogiston was the same thing with inflammable air; and he has sufficiently proved this from many experiments and observations, my own, as well as those of others. I did not, however, accede to it till I discovered it by direct experiments, made with general and indeterminate views, in order to ascertain something concerning a subject which had given myself and others so much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red-hot in flint glass tubes, gave them a black tinge, and was in a great measure absorbed, which I had discovered to be owing to the calx of lead in the glass, at-

tracting phlogiston from the inflammable air. As the quantity of air in these tubes was very small, though I gave it as my opinion, that the residuum in one of the processes, was phlogisticated air, because I perceived no marks of accension on presenting to it the flame of a small candle; I was not, on recollection, satisfied with this conclusion, and was desirous of repeating the experiment with more care, especially as, in one of the above-mentioned experiments, I found only a very small bubble of the inflammable air in the tube in which it had been heated.

I found, however, great difficulty in repeating these experiments; and the quantity of inflammable air operated upon in them, is necessarily so small, that the result is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glass, surrounded with inflammable air, or rather on the calx of lead alone, in the same circumstances, would be a much easier experiment, and might bring me nearer to my object; and on making the experiment it

immediately answered far beyond my expectation.

For this purpose, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient stand, introduced it into a large receiver, filled with inflammable air, confined by water. As soon as the minium was dry, by means of the heat thrown upon it, I observed that it became black, and then ran in the form of perfect lead, at the same time that the air diminished at a great rate, the water ascending within the receiver. I viewed this process with the most eager and pleasing expectation of the result, having at that time no fixed opinion on the subject; and therefore I could not tell, except by actual trial, whether the air was decomposing in the process, so that some other kind of air would be left, or whether it would be absorbed *in toto*. The former I thought the more probable, as, if there was any such thing as phlogiston, inflammable air, I imagined, consisted of it, and something else. However,

I was then satisfied that it would be in my power to determine, in a very satisfactory manner, whether the phlogiston in inflammable air had any *base* or not, and if it had, what that base was. For seeing the metal to be actually revived, and that in a considerable quantity, at the same time that the air was diminished, I could not doubt, but that the calx was actually imbibing something from the air; and from its effects in making the calx into metal, it could be no other than that to which chemists had unanimously given the name of *phlogiston*.

Before this first experiment was concluded, I perceived, that if the phlogiston in inflammable air had any base, it must be very inconsiderable: for the process went on till there was no more room to operate without endangering the receiver; and examining, with much anxiety, the air that remained, I found that it could not be distinguished from that in which I began the experiment, which was air extracted from iron by oil of vitriol, I was, therefore, pretty well satisfied that this inflammable air could not contain any thing

thing besides phlogiston; for at that time I reduced about 45 ounce measures of the air to five.

In order to ascertain a fact of so much importance with the greatest care, I afterwards carefully expelled from a quantity of minium all the phlogiston, and every thing else that could have assumed the form of air, by giving it a red heat when mixed with spirit of nitre; and immediately using it in the manner mentioned above, I reduced 101 ounce measures of inflammable air to *two*. To judge of its degree of inflammability, I presented the flame of a small candle to the mouth of a phial filled with it, and observed, that it made thirteen separate explosions, though weak ones (stopping the mouth of the phial with my finger after each explosion) when fresh made inflammable air, in the same circumstances, made only fourteen explosions, though stronger ones.

After this experiment I could not hesitate to conclude*, that this inflammable air went

* In this conclusion, I overlooked one obvious consideration, viz. that water, or any thing soluble in water,

totally, and without decomposition, into the lead which I formed at that time; and if the necessary circumstances of the experiment be considered, it will be thought extraordinary that, even admitting this, the result should be so decisively clear in favour of it: for, in the first place, the greatest care must be used to expel all air from the minium, and it must be used before it can have attracted any from the atmosphere; and in the next place, the water also (a considerable quantity of which must be used, and which will also be heated in the process) should be made as free from air as possible. In these circumstances, had I found the small residuum, of two ounce measures from 101, to have been phlogistigated or fixed air, I should not have been disappointed; and it would not have prevented my concluding that *phlogiston* was the same thing with *inflammable air*, contained in a might be the basis of inflammable air. All that could be absolutely inferred from the experiment was, that this basis could not be any thing that was capable of subsisting in the form of *air*. It will be seen, that I afterwards made the experiment with the air confined by mercury.

combined

combined state in metals, just as fixed air is contained in chalk and other calcareous substances; both being equally capable of being expelled again in the form of air.

Afterwards using a calx of lead, which had been prepared in the same manner with the former, but which had remained some weeks exposed to the air, I found, that when by using it I had reduced 150 ounce measures of inflammable air to 10, this residuum was phlogisticated air. But examining this calx separately, I found that it gave, by heat in a glass vessel, a considerable quantity of phlogisticated air.

I must observe, that the minium should not be reduced to a perfectly compact glass of lead; for then it would be too refractory to be easily revived by this process. Making use of some of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inside of the receiver: an experiment which I shall repeat and reconsider. I must also observe, that the lead which I procured in the above-mentioned process was not to be distinguished from any other

other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made use of inflammable air from wood, I found, that though I was able to reduce minium with it, it was effected with more time and difficulty. Forty ounce measures of this kind of inflammable air I reduced to 25; after which I found that the heat of the lens produced only *glass of lead*, and no *metal*. The air was still, however, inflammable; and there was a small mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have some reason to think, consists of an intimate union of fixed air with that which is of the *explosive* kind extracted from metals. The result of those experiments which I made with that kind of inflammable air which is collected in the process for making phosphorus, and which burns with a lambent yellow flame, was similar to those which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable result with inflammable air, I immediately tried all the
other

other kinds of air in the same manner; but in none of them did I procure any thing from the minium besides glass of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogisticated air, marine acid air, fluor acid air, as also in common and dephlogisticated air, I got no *metal* at all. In vitriolic acid air there was but a small quantity of lead produced, and I have observed that this kind of air imparts a certain portion of phlogiston to common air, rendering it in some measure phlogisticated, though by no means in so great a degree as nitrous air.

Though nitrous air and phlogisticated air certainly contain phlogiston, they appear by these experiments to hold it too obstinately to part with it to minium in this process, notwithstanding nitrous air quits it so readily to respirable air. I would observe, that there were some peculiar appearances in the experiments I made to revive the calx of lead in these kinds of air in which the attempt did not succeed; but I must repeat the experiments, and note the appearances.

appearances more accurately, before I report them.

In alkaline air lead seems to be formed from the minium as readily as in inflammable air, and indeed I thought rather more so; and this is a remarkable confirmation and illustration of those experiments, in which, by taking the electric spark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times since I first published the account of it, and always with the same result.

This experiment also throws some light upon those in which, by super-phlogisticating iron with nitrous air, I produced a strong smell of volatile alkali; an experiment which I have also frequently repeated with the same result. The reviving of lead in alkaline air may also help us to conceive how all *acids* should have an affinity both to *phlogiston* and to *alkalies*, which have hitherto appeared to be things so very different from

from each other; since, from these experiments, it is probable that one of them is some modification of the other, or a combination of something else with the other. To trace the connexion between the alkaline and inflammable principles, is a curious subject; and from these hints it may, perhaps, not be very difficult to prosecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound, and inflammable air, or phlogiston, the more simple substance of the two.

From $5\frac{1}{2}$ ounce measures of alkaline air I got, by means of litharge, 17 grains of lead, besides some that was dissolved in the mercury, by which the air was confined. There remained $2\frac{1}{2}$ ounce measures, which appeared to be phlogisticated air, and to have no fixed air in it. At another time, in eight ounce measures of alkaline air I got 15 grains of lead, besides what was dissolved in the mercury, which seemed to be a good deal in proportion to it. It was observable, that there remained in this process $3\frac{1}{2}$ ounce measures

*the former
 & Ash with
 some O₂*
 fures of phlogistified air, without any mixture of fixed air in it, though the massicot which I used at this time gave by heat only a good deal of pretty pure fixed air. These experiments with alkaline air well deserve to be resumed, and I shall not fail to do it at a proper opportunity.

Having thus produced *lead* in inflammable air, I proceeded in my attempts to revive other metals from their calces by the same means; and I succeeded very well with tin, bismuth, and silver; tolerably well with copper, iron, and regulus of cobalt; but not at all with regulus of antimony, regulus of arsenic, zinc, or the metal of manganese.

I was desirous also of ascertaining by this means the *quantity* of phlogiston that enters into the composition of the several metals; but in this I found more difficulty than I had expected; and this arose chiefly from the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not easy to revive the whole of any quantity of calx completely.

After

After many trials, I think I may venture to say, that an ounce of *lead* absorbs 100 ounce measures of inflammable air, or perhaps something more; for in one result it seemed to have imbibed in the proportion of 108 ounce measures.

An ounce of *tin* absorbs inflammable air in the proportion of 377 ounce measures to the ounce. An ounce of copper from verditer absorbed 403 ounce measures; from a solution of blue vitriol, precipitated by salt of tartar, and afterwards made red-hot with spirit of nitre, 640; but from blue vitriol itself 909 ounce measures. In this case, however, much of the inflammable air went to the formation of the vitriolic acid air, the smell of which was very perceivable in the course of the experiment. The copper that I made in this way was brittle, and therefore seemed not to be perfectly metalized; but being fused with borax, it became perfect copper, and, as I think, without any loss of weight.

Bismuth absorbed inflammable air in the proportion of 185 ounce measures to the ounce.

The

The calx I used was a precipitate from the solution of this metal in spirit of nitre.

Iron I got from a precipitate of a solution of green vitriol by salt of tartar, moistened with spirit of nitre, and exposed to a red heat. This calx absorbed in the proportion of 890 ounce measures of the inflammable air to an ounce of iron, which was in the form of a black powder; but to all appearance as much attracted by the magnet as iron filings. But it could not be expected, that perfect iron, containing its full proportion of phlogiston, should be produced in this manner, since inflammable air may be expelled from perfect iron in this very process*.

Silver I evidently revived from a solution of it in spirit of nitre precipitated by salt of tartar, and also from *luna cornea*. A quantity of this last substance absorbed 23 ounce measures of inflammable air; but I

* I have since found that inflammable air cannot be expelled from iron by heat, without some moisture, which therefore seems necessary to its constitution.

could

could not get any calx of silver free from small grains of the perfect metal, which was easily discovered by a magnifier, and therefore I could not ascertain the quantity of inflammable air absorbed by it.

Small grains of regulus of *cobalt* I produced from zaffre, and inflammable air was absorbed; but I did not estimate the quantity.

A quantity of *manganese* absorbed 7 ounce measures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpose, and mixing with it some calcined borax, I repeated the experiment, when there was again an evident absorption of air, and in the course of that experiment, I once thought that I did perceive a small globule of metal.

Zinc and *arsenic* were only sublimed in this process. The same was the case with the glass of *antimony*; but the experiment was attended with this peculiar circumstance, that when the glass was melted in inflammable air, it formed itself into needle-

like crystals arranged in a very curious manner; and I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a sufficient proof of its *containing* what has been called phlogiston; and its being absorbed by them *in toto*, without decomposition, is a proof of its being nothing besides *phlogiston in the form of air*, unless there should be something solid deposited from it at the same time that the proper phlogistic part of it is absorbed. With respect to this, I can only say that, in the course of the experiments, I did not perceive any thing of the kind: for though in some of the processes there was a black smoke produced, in others I could perceive nothing but part of the calx subliming, and clouding the glass. On this account, however, I could not pretend to ascertain the weight of the inflammable air in the calx, so as to prove that it had acquired an addition of weight by being metalized, which I often attempted. But were it possible to procure a perfect calx,

no

no part of which should be sublimed and dispersed, by the heat necessary to be made use of in the process, I should not doubt but that the quantity of inflammable air imbibed by it would sufficiently add to its weight.

Besides the formation of metals from their calces, I had other proofs, and of a nature sufficiently curious, of inflammable air containing phlogiston, though perhaps not sufficiently conclusive with respect to its being wholly and simply phlogiston itself. Thus, by means of it, I was able to make *phosphorus*, *nitrous air*, *liver of sulphur*, and *sulphur* itself, in all of which phlogiston is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glassy matter which is made from calcined bones by oil of vitriol in inflammable air, some of it was absorbed, and all the inside of the receiver was covered with an orange-coloured substance, which had a strong smell of phosphorus. I then wanted sunshine to continue the experiment; but I was satisfied that there was sufficient proof of phosphorus being actually formed in this manner.

manner. With alkaline air I succeeded much better.

In $2\frac{1}{2}$ ounce measures of this air, I produced, from the glassy matter mentioned above, two grains of phosphorus in one mass, the vessel being only filled with white fumes during the process. One-fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the process for making phosphorus.

That nitrous air contains phlogiston is sufficiently evident, if there be any such thing as phlogiston: and I have farther proved, that it contains as much phlogiston, in proportion to its bulk, as inflammable air itself. I have now, however, the farther satisfaction to be able to make nitrous air from its two constituent principles, viz. nitrous vapour and inflammable air. The most easy process for this purpose is, to throw a stream of nitrous vapour into a large phial previously filled with inflammable air. In this manner nitrous air is instantly formed, and in great quantities; but as this nitrous vapour is produced

by the rapid solution of bismuth in spirit of nitre, which at the same time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the same thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I first produced by uniting nitrous vapour to minium (in consequence of which, from being a red and powdery substance, it becomes white, compact, and brittle) I placed it upon a stand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two-thirds of the whole, and during this time lead was revived from the calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all strongly nitrous: and, from the circumstances in which it was produced, it must have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver.

In order to ascertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the space of 1,32 measures. Fresh nitrous air made in the usual way, and mixed with common air in the same proportion, occupied the space of 1,26. This difference arose not from any impurity in the nitrous air, but from the mixture of the dephlogisticated air, which is also expelled from this calx by heat.

Liver of sulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

Lastly, to produce *sulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen vessel, and evaporated it to dryness in a receiver filled with inflammable air; in consequence of which the inside of the receiver acquired a whitish incrustation, which when warmed had a strong smell of sulphur; and repeating the process in the same receiver, I was able, this second time, to scrape off enough of the matter to put on a piece of hot iron, and to produce

produce the genuine blue flame, as well as the peculiar smell of sulphur.

I shall conclude these observations on phlogiston with two articles; one of which seems to contradict an established maxim among chemists; and the other a former opinion of my own.

It is generally said, that charcoal is indestructible, except by a red heat in contact with air. But I find that it is perfectly destructible, or decomposed, *in vacuo*, and by the heat of a burning lens almost wholly converted into inflammable air; so that nothing remains besides an exceedingly small quantity of white ashes, which are seldom visible, except when, in very small particles, they happen to cross the sun-beam, as they fly about within the receiver. It would be impossible to collect or weigh them; but, according to appearance, the ashes thus produced from many pounds of wood, could not be supposed to weigh a grain. The great weight of ashes produced by burning wood in the open air, arises from what is attracted by them from the air. The air which I get in this manner is wholly inflammable, with-

out the least particle of fixed air in it. But, in order to this, the charcoal must be perfectly well made, or with such a heat as would expel all the fixed air which the wood contains; and it must be continued till it yield inflammable air only, which, in an earthen retort, is soon produced.

Wood, or charcoal, is even perfectly destructible, that is, resolvable into inflammable air, in a good earthen retort, and a fire that would about melt iron. In these circumstances, after all the fixed air had come over, I have several times continued the process during a whole day, in all which time inflammable air has been produced equably, and without any appearance of a termination. Nor did I wonder at this, after seeing it wholly vanish into inflammable air *in vacuo*. A quantity of charcoal made from oak, and weighing about an ounce, generally gave me about five ounce measures of inflammable air in twelve minutes*.

*Notwithstanding these facts, it will appear from my subsequent experiments, that water was necessary to the formation of this inflammable air from wood as well as of that from iron,

The second article that I shall now mention, affords an indisputable proof of the generation of fixed air from dephlogisticated air and phlogiston, or inflammable air. I have several times given it as my opinion, that fixed air is a *facitious substance*, and a modification of the nitrous and vitriolic acids, my former experiments greatly favouring that conclusion; but that it was composed of dephlogisticated air and phlogiston, though maintained by my friend Mr. KIRWAN, I was far from being satisfied with, till I was forced to consent to his proof of it from my own former experiments, and gave him leave to mention it, as he has done in his late excellent paper on salts. But I have lately had two direct proofs of it by experiment.

The first was when, in repeating a beautiful experiment, first made by Dr. INGENHOUSZ, but with some variation, I was firing some shavings of iron in dephlogisticated air confined by mercury, by means of a burning lens. In this way I quickly fired the iron, and it burned away in a very pleasing manner.

ner. But what struck me most was, that, of the air that remained, a considerable portion was fixed air, though in the receiver I had nothing but the purest dephlogisticated air, together with the iron, which could only give inflammable air. I would observe, that the melted iron formed itself into large balls, which appeared to be a mere *slag* or *glass**, and was no longer iron.

Afterwards, to put this hypothesis concerning the constituent principles of fixed air to a more direct proof, I mixed iron filings, which give only inflammable air, with red precipitate, which I found to give nothing but the purest dephlogisticated air; and when I heated them in a coated glass retort, they gave a great quantity of fixed air, in some portions of which, nineteen-twentieths were absorbed by lime-water, but the residuum was inflammable. However, when I mixed with the red precipitate a quantity of powdered

* The nature of what is here called *slag* or *glass*, will appear in the subsequent experiments; being the same that is called *scales of iron*, and also *finery cinder*, or the calx of iron combined with water.

charcoal,

charcoal, which I had found to give only inflammable air, the fixed air produced from it was so pure, that only one-fortieth part of it remained unabsoꛑed by water ; so that this fixed air was as pure as that which is generally procured from chalk by oil of vitriol.

It appeared, in some of these experiments, that three ounce measures of dephlogisticated air go into the composition of two ounce measures of fixed air. For one ounce of this red precipitate gave 60 ounce measures of dephlogisticated air ; and when mixed with two ounces of iron filings, it gave about 40 ounce measures of fixed air that were actually absoꛑed by water, besides a residuum that was inflammable*. I had the same proportion when I used half an ounce of each of the materials. But when I used one ounce of each, I got only 20 ounce measures of fixed air, including the residuum. At other times

* It must be observed, however, that part of the fixed air is always imbibed by the water in which it is first received. Otherwise, in this experiment, the fixed air would have weighed no more than the dephlogisticated air in the composition of it, so that nothing would be left for the inflammable air.

I had

I had different proportions with different quantities of iron filings and charcoal.

I cannot conclude these observations without taking notice, how very valuable an instrument in philosophy is a good burning lens. This must have been perceived in many of my former experiments, but more especially in these. By no other means can heat be given to substances *in vacuo*, or in any other kind of air besides atmospherical; and without some method of doing this, no such experiments as these can possibly be made. I therefore congratulate all the lovers of science on the successful attempt of Mr. PARKER to execute so capital an instrument as he has done of this kind. Such spirited and generous exertions reflect honour on himself, and on our country. It is only to be wished, that we could have lenses of a smaller size (*viz.* from 12 to 18 inches diameter) made tolerably cheap, so that they might be in more common use. All my experiments were made with one of 12 inches in diameter.

S E C-

SECTION II.

*Experiments relating to the seeming Conversion
of Water into Air*.*

SINCE many persons have expressed a wish to be acquainted with the experiments I have lately made, which at first seemed to favour the idea of a *conversion of water into air*, but which terminated in the discovery of a fact, in my opinion, still more extraordinary, I shall submit to the Royal Society the result of the observations I have already made; though, as yet, I have by no means been able to satisfy myself so fully as I could wish with respect to some particulars connected with the subject. All the *facts* which I shall state may be depended upon; but it is probable, that different per-

* This section is taken from the Philosophical Transactions, vol. lxxiii. p. 414.

sons may draw different *conclusions* from them; and to mere *opinions*, I have never shewed myself much attached.

Having formerly observed several remarkable changes in fluid substances, in consequence of long exposure to heat in glass vessels hermetically sealed (of which an account may be seen in the fourth volume of my *Philosophical Observations*) I then formed a design of exposing all kinds of solid substances to great heats, in a similar state of confinement; and for that purpose provided myself with a cast-iron vessel, which I could close at one end, like a digester, and of such a length, that one of the ends might be red-hot, while the other was sufficiently cool to be handled. To this end there was a cock connected to a tube, by means of which I could let off steam, or air, in any period of the process.

I imagined, that when substances, consisting of parts so volatile as to fly off before they had attained any considerable degree of heat, in the usual pressure of the atmosphere, were compelled to bear great heats under a
greater

greater pressure, they might assume new forms, and undergo remarkable changes, similar to what we may suppose to be the case within the bowels of the earth, where, by means of subterraneous fires, various substances bear great heats under very great pressures.

I have had this instrument some years; but it was so ill constructed, that I could not make the use of it that I had originally intended. I therefore lately fitted up some gun-barrels in the same manner, and made my first experiment with lime-stone; expecting, that when the fixed air, and other volatile matters, that might be contained in it, should be compelled to bear a red-heat, without a possibility of making their escape, the substance itself might undergo some change; but I had no particular expectation concerning the nature of that change. I had, however, been so often favoured with valuable results, from merely putting things into new situations, that I was encouraged to make the experiment; but I found an unexpected difficulty in getting a cock that would be air-tight

tight and steam-tight, under so great a pressure as I wished to apply.

I was mentioning these ideas to Mr. WATT, in whose neighbourhood I have the happiness to be situated, when he mentioned a similar idea of his, *viz.* that of the possibility of the conversion of water, or steam, into permanent air; saying, that some appearances in the working of his fire-engine had led him to expect this. He thought that if steam could be made red-hot, so that all its latent heat should be converted into sensible heat, either this or some other change would probably take place in its constitution. The idea was new to me, and led me to attend more particularly to my former projects of a similar nature, and I began with lime-stone, wishing to try the effect of giving a red-heat to lime, in which water only should be previously combined, thinking it might possibly have the same effect with making the water itself red-hot.

Accordingly, I took a quantity of well calcined lime, and mixing with it a little water, out of which all air had been carefully boiled,
I exposed

I exposed it gradually to a strong heat in an earthen retort, such as I had been usually supplied with by Mr. WEDGWOOD (who is as much distinguished by his love and generous encouragement of science, as he is by his improvements in his own curious art) not imagining that it could make any difference whether the lime, so prepared, should receive its heat in an earthen retort, or in a vessel of iron or glass. Proceeding, however, in this manner, I found that nothing came over in the form of *steam*, but that there was a great quantity of *air*, several hundred times more than the bulk of the water, and at that time there was in it a considerable proportion of fixed air, which I imagined might either be that which had not been sufficiently expelled before, or might be composed of some phlogistic matter contained in the lime, and the purer air that was yielded by the water. For I own I then concluded, that the air which I got (and which, when the fixed air was extracted from it, was such as a candle would just burn in) came from the water, especially as in some of the processes, the weight

of the air I caught was very nearly, if not quite equal to that of the water, and interposing a large glass balloon between the retort and the recipient for the air, I observed that it remained perfectly cool and dry during the whole process; and several hours afterwards there was not the least moisture condensed in it. I also received a quantity of another produce of air, made in this manner in mercury, and having viewed it with the greatest attention, observed that, after several days, it never deposited the least moisture.

I then calcined a quantity of natural limestone, with this glass balloon interposed in the same manner, and found no water, but only air to come from it, though the stone is generally supposed to contain water. But when I used much more than half an ounce of water to the quantity of whiting or lime above-mentioned, I always had some water come over, though very little in proportion to the quantity made use of.

I did not fail to examine whether there had been any loss in the weight of the lime, or whiting, in order to determine whether any

part

part of these solid substances had entered into the composition of the air; but I found much difficulty in weighing them with exactness, after shaking them out of an earthen retort, into which I could not see, and to which part of these earthy matters often adhered, so that I could not obtain much satisfaction even when I broke the retort. Besides, there was always some loss of the earth in the cloudiness of the air; whenever the production of it was rapid. In a future process I had abundant proof that the air did not come from any earthy matter with which the water had been combined.

Hitherto I had no idea but that all that was necessary to the conversion, as I concluded it to be, of water into air, was to give it a red heat, without which it would not quit the calcareous earth; and I imagined that by this means the matter, or principle, of *heat* was so intimately combined with it, as not to be separated from it by cooling, as in the case of steam. But I, as well as all my friends, was a long time utterly disconcerted upon finding that when I

put the whitening and water into a coated glass retort, the water came over in the form of steam, and little or no air was produced. The result was also the same when I made the process in a gun-barrel, in a porcelain retort, or even in an earthen retort glazed in the inside.

That the earth had not lost its property of doing its part in the business, I found by putting more water to the same whitening which had failed in the glass retort, and which had been used no less than four times before, and then heating it in an earthen retort; when again it gave air only, and no water, the same as before. And at this time I observed, that part of the air was hardly to be distinguished from that of the atmosphere.

I cannot express my surprise at my unexpected failure with the glass retort; and my speculations on the subject were various, but at that time altogether ineffectual. Among other things it occurred to me that, possibly, some phlogiston, either contained in the earthen retort, or coming through it (though

I could

I could not tell *how*, or on what principle) from the fire, might be necessary to water, and all other substances assuming the form of air. But when, with this idea, I put spirit of wine, oil, or iron-filings to the lime, I got nothing from these mixtures in glass retorts besides steam and inflammable air, from the decomposition of these substances containing phlogiston.

That there was nothing in the materials of which the earthen retort was made, that necessarily produced the air, was evident from my not succeeding when I pounded a broken retort, and heated it, mixed with water, in one of glass.

Being satisfied that the production of air depended very much upon the retort itself, I thought of using the retort only with water, but without any lime, or earthy substance; and I found it succeed far beyond my expectation. For when I put a small quantity of water into one of these retorts, and endeavoured to distil it gently, I never failed to procure about an hundred ounce measures of air; and this I could do as often as I pleased,

with the same retort, and without its losing any weight; and the air produced in this manner never had any portion of fixed air in it, and was always but very little inferior to that of the atmosphere.

In all these processes I observed, that very little of this air was procured till all the water that could be poured out of the retort was evaporated, for the difference in the produce was very little; whether I exposed the retort to the fire quite full of water, or with only about an ounce measure of water in it, or even after letting it remain full for a short time, and then pouring out all that I could from it; so that it was only that water which was entangled, as it were, in the pores of the retort, and which had been in some measure united to the substance of it, that had contributed to this production of air.

These retorts (which Mr. WEDGWOOD informs me are made of a mixture of fresh and of burnt Devonshire pipe clay) are pervious to water, though not to air; so that while the air is produced from that water which has entered the pores, the rest is sometimes visibly making

making its escape in the form of a copious smoke on the outside. It was evidently impossible, however, and contrary to all the laws of hydrostatics, that air should enter by the same pores by which the water or steam was escaping, and at the same time that its endeavour to force its way out of the retort was such that it overcame a considerable resistance from the column of water, at the mouth of my recipients. Air might have escaped through any unobserved pores in the retort, but none could have entered that way: and if there was the least sensible crack in any part of the retort, I was never able to collect any air at all.

But the following experiments may, perhaps, shew that it is sufficient for the production of air that steam come into contact with clay sufficiently heated. Between a copper still and the glass tube communicating with my recipient for air, I introduced the stem of a tobacco-pipe; and by means

* It will appear, however, that the air must have entered by the pores, but by means of a power very different from that of pressure, and able to counteract it.

of a small furnace, I kept about three inches of the middle part of it moderately red-hot. In this state, making the water boil, I uniformly received air, though mixed with steam, at the rate of five ounce measures in twelve minutes for more than an hour; but when I let the pipe cool, nothing but steam was delivered by it without any air at all. There was no fixed air in this produce, and it was as ill such as a candle would hardly have burned it. It might, I thought, have been better, and also more in quantity, if I had not used the stem of a foul pipe. But when I used a clean pipe in the same manner, I did not find the air much, if at all, improved. Suspecting this to arise from the near contact of the fuel, I inclosed the tobacco pipe in an earthen tube, and then I had air as good as I had generally got in the earthen retort, and not much worse than that of the atmosphere. that I had drawn from it

Another circumstance I observed was, that if the outside of the vessel which contained the water or steam, through which it passed, when the requisite heat was applied to it,

was not dry, or perhaps surrounded with good air (for in those circumstances the following experiment differs from the preceding ones) the experiment did not succeed.

When I put the ball of an earthen retort, filled with moist clay, into an iron digester, and applied heat to it, I got only a very little fixed air (which was probably composed of a small quantity of air beginning to be produced from the materials) and inflammable air from the vessel. All that came over besides was steam, and at last inflammable air from the vessel itself.

Being now able to procure air by means of water, in this most simple method, *viz.* by water only in the earthen retort, I had an opportunity of ascertaining, with great ease and exactness, several circumstances relating to the process, and of obviating, as I thought, some objections to the conclusion that I had drawn from it. Among other things, I fully satisfied myself that the *earth of the retort* contributed nothing at all to this production of air, but the *water only*: for having used the same retort till I had
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got from it nearly an ounce weight of air, or 800 ounce measures, I found that it had not lost so much as a single grain in weight. After the first process it weighed just three grains more than it did at first, and it continued to weigh the same till after the last process. This small addition of weight might easily have come from a little of the water having been imbibed by the neck of the retort, where the heat of the fire could not reach it. When all the processes were over, I kept the whole retort in a red heat for several hours, and then found that, besides losing those three grains, it weighed eight grains less than it did at first.

Before this I had found, that the calcined whiting which I had used in the first experiment could not, as some had supposed, attract from the atmosphere, any considerable part of the air which I got from it, after combining water with it: for two ounces of the whiting (which was the quantity that I generally made use of) did not attract more than eight grains of any thing when it was exposed a whole day in an open dish, though
it

it had lost more than half its weight in calcination. It has been imagined by some, that the air which I got in these earthen retorts was that which had been attracted from the atmosphere by the inside surface of them. But, besides that no air could ever be produced without water, to obviate this objection more particularly, when one of these retorts was giving its last portion of air, I immersed the mouth of it in a basin of water; and letting it cool in that situation, filled it again without admitting any access of air to the inside; and yet, on repeating the process with it, the air was produced just as freely as before. This operation I repeated several times. If it be said, that the outside of the retort attracted the air, still the inside, being composed of the same materials, must have attracted air also; and it would have appeared by the ascent of the water from the basin, the retort being sufficiently impervious to air. By some it was imagined, that either the air itself that I procured, or at least the power

power of the retort to contribute to the production of it, was owing to something that was transmitted from the burning coals, but which could not pass through glass or metals. To determine this, I took an earthen tube, of the same composition with the retort, and putting a little water in it, placed it, surrounded with sand, in a glass vessel, and this again, surrounded also with sand, in an iron one; and yet the heat transmitted through all these substances, enabled the earthen tube to give air, in the same proportion, and of the same quality, as it would have done if it had been exposed to the naked fire.

Having now procured air, by means of water, in a very simple, and as I thought, an unexceptionable manner, I wished to make it in greater quantities, in proportion to the water employed; and for this purpose I first thought of increasing the size, or the thickness of the porous retorts: but I thought it might answer as well if I put into the retort, in powder, the materials of which they were made, or other substances of the same kind.

Accord-

Accordingly, by mixing ground flint and clay in various proportions, I presently increased the quantity of air much beyond my expectation. In the first trials, in which I had much flint and a little clay, I never failed to get 200 ounce measures of air from one of water. Then, using more clay and less flint, I had still more air; and at last, leaving out the flint altogether, and using clay only, I never failed to get much more than 400, and generally between 500 and 600 ounce measures of air from one of water, which was about three-fourths of the weight of the water; and in one particular process I procured very little less than nine-tenths of the weight of the water in air, and this air was never much less pure than that of the atmosphere. Sometimes it could not be distinguished from it at all by the test of nitrous air; and once or twice I thought it even purer than that of the atmosphere.

I must here observe, that I found it not convenient to put so much water to any quantity of clay as would make it cohere in one mass, but only so much as that it should remain

remain in the form of powder. By this means it might easily be poured out of the retort when the experiment was over.

The weight of the water expended in this production of air I ascertained, in the most unexceptionable manner, by weighing the retort, with all its contents, before and after the process. I shall explain this by the result of two of the processes. In one of them, the retort and moistened clay together lost in weight 588 grains, after yielding 741 ounce measures of air, which (in the proportion of six grains to one ounce-measure) would have weighed 444 grains, and consequently, about three-fourths of the weight of the water.

In the other process, the loss of weight was 358 grains, after yielding 556 ounce measures of air, which would have weighed 333 grains. The proportion, therefore, between the weight of the air and that of the water was 111 to 116, or nearly nine to ten.

I also found now, that so much heat as I had hitherto applied was neither necessary nor useful. In the last-mentioned process

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the retort was constantly suspended about six inches above a moderate charcoal fire; at another time more than twelve or fifteen inches above it, where a FAHRENHEIT'S thermometer did not shew more than 210° . With this moderate heat I got 465 ounce measures in the course of about twelve hours. When the retort was suspended within six inches of the fire, the air was generally produced at the rate of 30 ounce measures in five minutes. But a thermometer, the bulb of which was immerfed in the clay, was still only at the heat of boiling water.

In all these processes, however, there was evidently some loss of water; for, excepting the first experiment with the lime, I never got the whole weight of the water in air; and it might be said that I only expelled the air before contained in the water, though from these experiments it appeared to contain much more air than it had been thought capable of containing. To obviate this objection, I contrived to catch all the water that escaped through the pores of the retort in the following manner.

Having

Having put the moistened clay into an earthen tube, to which I had fitted a cock and a long glass tube (by means of which I could collect all the air that came from it) I put this within an iron tube, which was closed at the end next the fire, but open at the other end, and so long that I could easily keep this open, and quite cool, while the other was in the fire; consequently, whatever water escaped through the pores of the earthen tube, it would be condensed in the cool part of the iron one. This water I carefully collected, and always found that the weight of it, together with that of the air produced in the experiment, was nearly that of the original weight of the water, estimated by the loss of weight in the earthen tube and its contents. I also found, that the water so collected served for the production of more air, just as well as any other water whatever, so that there had been no decomposition of the water in the case.

In the last process that I went through of this kind, the loss of weight in the earthen tube, or rather of the water contained in it,

was 392 grains; the air collected was 173 ounce measures, which would have weighed 89 grains, and the water which escaped through the pores of the earthen tube, and which I collected, was nearly 195 grains; so that the air and this water together weighed 302 grains, or ten grains more than the original water. But as I estimated the weight of the water only by the space which it occupied in a cylindrical glass tube, divided according to ounces and parts of ounces of water, it was not easy to avoid an error of a few grains. At other times there was an error of a small magnitude on the other side. But it will appear hereafter, that more steam must have escaped invisibly at the open mouth of the iron tube than I was aware of.

That nothing could enter by the pores of the retort at the same time that the water was making its escape out of them, I thought I ascertained pretty satisfactorily by immersing the bulb of it in mercury, contained in an iron vessel. In these circumstances I obtained air as usual, only the produce was

not so rapid. In this way, however, I procured above an hundred ounce measures of air from moistened clay, and I discontinued the process without perceiving any termination of it. But the moment the retort was raised out of the mercury, it gave air three times as fast as it had done before. The quality of the air was the same as the both cases, viz. a little worse than that of the atmosphere. I even collected thirty ounce measures of (air when the bulb of the same retort was immersed in hot linseed oil, but the production of air gradually ceased, and the next day I found the retort almost full of the oil, which had soaked through it. Distilling this oil, I got 300 ounce measures of air wholly inflammable, except a very few ounce measures at the last, which were only phlogisticated.

Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. CAYENDISH's concerning the re-conversion of air into water, by decomposing it in conjunction

junction with inflammable air. And in the first place, in order to be sure that the water I might find in the air was really a constituent part of it, and not what it might have imbibed after its formation, I made a quantity of both dephlogisticated and inflammable air, in such a manner as that neither of them should ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the process (long after the water of crystallization was come over) and the latter from perfectly made charcoal. The two kinds of air thus produced, I decomposed by firing them together by the electric explosion, and found a manifest deposition of water, and to appearance in the same quantity as if both the kinds of air had been previously confined by water.

In order to judge more accurately of the quantity of water so deposited, and to compare it with the weight of the air decomposed, I carefully weighed a piece of filtering paper, and then having wiped with it all the inside of the glass vessel in which the air had

been.

been decomposed, weighed it again, and I always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper.

As there is a source of deception in this experiment, in the small globules of mercury, which are apt to adhere to the inside of the glass vessel, and to be taken up by the paper with which it is wiped, I sometimes weighed the paper with the moisture and the mercury adhering to it; and then exposing it in a warm place, where the water would evaporate, but not the mercury, weighed it again, and still found, as nearly as I could pretend to weigh so small a matter, a loss of weight equal to that of the air. I wished, however, to have had a nicer balance for the purpose: the result was such as to afford a strong presumption that the air was re-converted into water, and therefore that the origin of it had been water.

Another presumption in favour of the generation of our atmosphere from water was, that the purity of the Air that I produced from it is so very nearly the same with that

of the atmosphere. And the degree of heat requisite to produce it is no greater than may be given by the rays of the sun in certain circumstances. Subterraneous fires, however, would be abundantly sufficient for the purpose, as it appears to be sufficient for the conversion of water into respirable air, that it come into contact with clay, and perhaps many other earthy substances in the form of vapour. I must, however, observe, that when I threw the focus of a burning lens upon a quantity of moist clay, either *in vacuo*, or in common air, I got no air from it.

I made this experiment both with the clay exposed in an open dish, and also confined in a short earthen tube. Had I then proceeded to repeat this last process with a communication between the inside of the earthen tube and the external air (as I then proposed to do, but was prevented) I should much sooner have discovered what I did afterwards, *viz.* that there was no real conversion of water into air in this process. In favour of which, however it may not be amiss to observe, that the great difficulty Mr. Dr. Lue and others have found in expelling all air from water,

is best accounted for on the supposition of the generation of air from water, though in other circumstances than those that I have observed. I have the pleasure to add, that Mr. De Luc himself concurs with me in this opinion.

The difficulty that strikes many persons most forcibly, is the want of analogy between the conversion of water into air with any other known facts in philosophy or in nature. But admitting that this conversion is effected by the intimate union of what is called the *principle of heat* with the water, it appears to me to be sufficiently analogous to other changes, or rather combinations, of substances. Is not the acid of nitre, and also that of vitriol, a thing as unlike to air as water is, their properties being as remarkably different? And yet it is demonstrable, that the acid of nitre is convertible into the purest respirable air, and probably by the union of the same principle of heat.

It will appear by subsequent experiments, that there is no acid in dephlogisticated air, this being only one of the component parts of water.

It is true, that steam is a thing very different from air, and I find that it is not able to decompose nitrous air; but then, though it has acquired sensible heat, it has got no latent heat so intimately combined with it as it is with air; and for the same reason, perhaps, the vapour of nitrous acid is not dephlogisticated air.

By the same process by which respirable air seems to be made from water, inflammable air may certainly be made from liquid substances containing phlogiston. Making spirit of wine to boil in a glass retort, I made the vapour pass through the stem of a hot tobacco-pipe, and found that it was all converted into inflammable air, and it was of that kind which burns with a lambent white flame. But when I let the pipe cool, no air was produced, but only vapour, which was instantly condensed in the water.

Being now master of a new and easy process, I was willing to extend it to other liquid substances; and I presently found, as I then imagined, that, by this means, I

could

could give a permanent aerial form to any liquid substance that had been previously thrown into the form of vapour. When I made the vapour of spirit of nitre, heated in a glass retort, pass through the stem of the hot tobacco pipe, I got as pure dephlogisticated air as ever I have procured from nitre; though the cork, by which the retort was connected with the pipe, was dissolved, and must have contributed to contaminate it, and give it a slight mixture of fixed air.

With oil of vitriol I got air considerably phlogisticated, so that a candle would not have burned in it; but this I also attribute to the cork, which was dissolved in the process. The result was nearly the same when I used water impregnated with vitriolic acid air, though the cork was not dissolved. But this acid is known to contain much phlogiston.

Spirit of salt gave air no purer than the best atmospherical air. But as by this process I never got air so pure as this from

water only, I concluded, that even this acid as well as the nitrous and vitriolic is capable of being changed into dephlogistinated air. When I used water impregnated with fixed air, this air was expelled by the heat, and came over without any change that I could perceive, except that the residuum was larger, from the water that came along with it. The air I got afterwards was only that from the water, and of the same quality as if it had not been impregnated with fixed air.

Water impregnated with alkaline air gave neither fixed nor inflammable air, which I had rather expected, but only air considerably phlogistinated; though some of it was so pure that a candle would have burned in it.

N. B. In all these experiments with the tobacco-pipe, all the air was remarkably turbid, like milk, and even the common air in the retort before the process properly began. In this state of the experiments I think I may venture to say, that no person could have

have seen them without concluding that there was a real conversion of water into air; there being no known principle or fact in philosophy, that could have led any person to suspect a fallacy in the case. In this, I therefore, I must have acquiesced, as indeed did all my acquaintance, even those who had been the most incredulous on the subject, after they had themselves seen the experiments. But I was led to the farther prosecution of this business, in consequence of having observed that the purity of the air which I procured depended upon the state of that which was immediately contiguous to the earthen retort, or tube, in which I supposed the conversion to have been made; and that some communication with the atmosphere was necessary to the production of any air, as in the experiment with the digester, and those with the clay and the burning lens. And since pure external air was necessary in order to procure good air, it was concluded by several of my friends, and especially Mr. WARR, that the operation of the earthen retort was, to transmit phlogiston from the water
or vessel con-

contained in the clay to the external air, and that the water, thus dephlogisticated, was incapable of being converted into respirable air by the intimate union of the principle of heat. In order to ascertain what the influence of the external air in this case really was, I inclosed an earthen retort filled with moistened clay, in a large glass receiver, open at both ends, through the upper orifice of which (being narrow) I thrust the neck of the retort, luting it so as to be perfectly air-tight; and placing the receiver in a basin of water, by which the air within was cut off from all communication with the external air, I fitted to the mouth of the retort a glass tube, through which I could receive whatever was produced in the process. In this situation I heated the retort, by means of Mr. FARGIER's excellent burning lens, when air was received through the tube communicating with the inside of the retort as usual; but at the same time the water rose within the tube. Fig. 1 exhibits a view of the disposition of the apparatus in this experiment, and in many others of the same nature.

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the receiver. This effect might be owing to a phlogistication of the air within the receiver; but it was soon diminished far beyond the utmost limit of that process, so that very little of it remained; and examining this air, I found it to be nearly the same with that of the atmosphere, as also was that which came from the retort.

This experiment made it probable, that the air on the outside of the receiver had actually passed through it; and yet it was contrary to all the known principles of hydrostatics, and even any thing hitherto known in chemistry, that air should be transmitted through a vessel of this kind, and in a direction contrary to that in which it would have been forced by the pressure of the atmosphere; while the water, with which the clay was moistened, went the other way. For had the retort been pervious to air, as the inside had a free communication with the atmosphere, the water could not have risen within the receiver. This, however, appeared to be the case, by the following decisive experiments.

Having

Experiment relating to the
Conversion of Water into Air.

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Having filled the earthen retort with the moistened clay as before, I made the inside of the receiver perfectly dry, and placed it in a basin of mercury; when, upon heating the retort as before, the receiver was all covered with dew, which collecting into drops trickled down the inside of the receiver, and remained upon the mercury, which rose within the receiver, while air was received from the retort as usual. I had no doubt, therefore, but that all the water within the retort would have got through into the receiver. Spirit of wine, or some thing that had the smell of it, was transmitted from the clay through the retort in the same manner.

I then filled the receiver with inflammable air, and upon heating the retort, it was all drawn through it, and delivered as strongly inflammable as ever by the tube communicating with the inside of it; while the water rose within the receiver, and even covered the retort, which was fixed at the very top of it, so that hardly any of the inflammable air remained within it. In like manner

manner nitrous air passed through the retort unchanged.

From these experiments it seemed impossible not to infer, that the clay of the earthen retort, being thus heated, destroys for a time the aerial form of whatever air is exposed to the outside of it, which aerial form it recovers after it has been transmitted in combination from one part of the clay to another, till it has reached the inside of the retort, while the water is drawn through it in a contrary direction.

Had this hypothesis been proposed *a priori*, it would, I doubt not, have been thought more extraordinary than the conversion of water into air. I propose to make many other experiments in the prosecution of these; but till I have an opportunity of doing this, I shall not trouble the Society with any conjectures that have occurred to me on the subject.

* Notwithstanding this, reasons will presently be given, which shew that this is no just conclusion, and that the process must have been carried on by means of pores.

The

Conversion of Water into Air. 63

The great difficulty with respect to the experiment with the lens is, that the water should pass through the retort one way, and the air the other, and yet that the air should not be able to pass without the water. It is also not a little extraordinary, that the weight of the air and that of the water should be so nearly equal.

In the last place I must observe, that there is nothing in this experiment that contradicts the idea of the conversion of water into air, though it does not prove it; for still the experiment with the tobacco-pipe, in which the steam is made red-hot (whereas in that with the lens it is only of a boiling heat) cannot be explained so well on any other hypothesis any more than Mr. CAVENDISH's experiment on finding water on the decomposition of air.

I cannot conclude this account without acknowledging my obligation to Mr. PARKER for the use of his incomparable lens, and his obligingly assisting me in the management of it. Indeed, without this very instrument, or one of greater power than my own,

own, I do not know that the last-mentioned experiments could have been made at all; certainly not to so much satisfaction*.

Experiments on the seeming Conversion of Water into Air continued.

WHEN I wrote the preceding article, it was my opinion, that the air which was transmitted through the earthen retort, had lost for a moment its *form of air*, and was properly incorporated with the *earth*, as fixed air is in limestone, and that it had resumed its aërial form in the inside of the retort; because these vessels had been air-

* By means of small earthen vessels, of an inch diameter in the bulb, such as is represented in fig. I. the experiment is made with sufficient advantage, with a lens of 12 inches in diameter.

tight

tight in all my other experiments. But though these earthen retorts be sufficiently air-tight for many purposes, and we cannot blow through them when they are plunged in water, yet unless they be *glazed*, they are not so air-tight, but that, with an air-pump, air may be drawn through them. Consequently, any power acting with the force of an air-pump (which is only equal to the pressure of the atmosphere) may overcome the difficulty of air passing through the small pores of such retorts; and there are many powers in chemistry much greater than this.

At present it is my opinion, that the agent in this case is that principle which we call *attraction of cohesion*, or that power by which water is raised in capillary tubes. But in what manner it acts in this case, I am far from being able to explain. Much less can I imagine how *air* should pass one way, and *vapour* the other, in the same pores, and how the transmission of the one should be necessary to the transmission of the other. I am satisfied, however, that it is by means of such pores as air may be forced through,

that this curious process is performed, because the experiment never succeeds but in such vessels as, by the air-pump at least, appear to be *porous*, though in all such. But the smallness of the pores seems to be rather an advantage in this experiment; and the smallest capillary tubes also are known to act with the greatest force.

A vessel scooped out of *chalk* answered as well as an earthen retort; so also did one of *free-stone*, first about a quarter of an inch in thickness, and then very near an inch, which I got made for the purpose; and so also did a vessel of *white marble* of the closest texture, though it is sufficiently air-tight for most purposes, and nothing but the air-pump could have convinced me that it had any pores.

In order to measure the force of this attraction, I placed the glass vessel through which I transmitted the rays of the sun (and which contained the air that was to be drawn through the retort) in a basin of mercury, and found that while the vapour of water was transmitted from moistened clay

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in the inside of the retort, into the inside of the glass vessel (and consequently the air passed the contrary way, so as to enable the atmosphere to press the mercury into the inside of the glass vessel) it rose $3\frac{1}{2}$ inches above the level on the outside of the glass. It might probably have risen something higher, if all the water in the clay had not then been expended. This was a vessel made on purpose, of pipe-clay only, and very compact. When I used more porous earthen vessels, I was never able to raise the mercury so high. For when the pores are large, the mechanical action occasioned by the weight of such a column of mercury counteracts the chemical one, and prevents the transmission of the air or steam.

Having sufficiently ascertained what kind of vessels would answer for this experiment, I then tried what kind of *fluid substances* would, by their transmission in one direction, promote the passing of the air in the other; and I found that *oils*, as well as *spirit of wine*, answered this purpose

perfectly well; and so also did *mercury*, but not any other metal made fluid by heat.

The mercury passed through the retort in the form of a subtle *black powder*; and on this occasion I observed that the vessel of pipe-clay with which the experiment was made, was so far air-tight, that when I took it to the air-pump, nearly full of mercury, quite hot from the experiment in the sun, and placed it upon the plate of the air-pump, I was not able to force any mercury through it. Probably this black powder was much more attenuated than mercury in its natural fluid state can be. There is also a degree of repulsion between white mercury and all glass or earthen vessels. This repulsion mechanical pressure might not be able to overcome, though this chemical action could do it. It remains, however, to be considered, how the mercury in this process could be changed from its natural fluid state into this black powder.

While I was making these experiments with mercury, I took the opportunity of trying

trying one of the kinds of air that cannot be confined by water; and having filled the glass vessel with *alkaline* air, and having mercury in the vessel of clay; I soon found, by the smell, that the alkaline air was passing one way, while the mercury went the other, just as readily, I doubt not, as inflammable or nitrous air had gone one way, while water had gone the other.

Of the *metals* in a state of fluidity, I tried *lead*, *tin*, *zinc*, and *bismuth*, melting them in porous earthen retorts; but neither did any metal pass one way, or air the other.

I made some attempts to perform this experiment with vessels which had *visible pores*, especially with fine capillary tubes of glass; having the stoppers of the glass tubes made with fine perforations of this kind, but hitherto without success; and yet I do not absolutely despair of succeeding in an experiment of this kind, and thereby seeing a little farther into this curious process.

SECTION III.

Experiments and Observations relating to Air and Water.*

EVER since the discovery of the diminution of respirable air in those processes which are generally called *phlogistia*, it has been a great object with philosophers to find what becomes of the air which disappears in them. Among others, I have made and published a variety of experiments with that view; but though by this means some farther progress was made in the philosophy of air, and consequently our knowledge of the principles, or constituent parts, of natural substances was extended, I did not by any means succeed to my satisfaction with respect to the immediate object of my researches. Others, however, were more successful, and their success has at length enabled me to resume my experiments with

* This section is copied from the Philosophical Transactions, vol. lxxv. p. 279.

more advantage; by which means I have been led both to confirm their conclusions, and, by diversifying the experiments, to throw considerable light upon various other chemical processes. The result of these observations I shall lay before the Society, with as much brevity and distinctness as I can.

In the experiments of which I shall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. CAVENDISH, Mr. WATT, and M. LAVOISIER. Mr. CAVENDISH was of opinion, that when air is decomposed, water only is produced; and Mr. WATT concluded from some experiments, of which I gave an account to the Society, and also from some observations of his own, that water consists of dephlogistated and inflammable air, in which Mr. CAVENDISH and M. LAVOISIER concur with him; but Mr. LAVOISIER is well known to maintain, that there is no such thing as what has been called *phlogiston*, affirming inflammable air to be nothing else

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but one of the elements or constituent parts of water. In the following experiments I also had a particular view to a conclusion which I had drawn from those experiments, of which an account is given in my last communications to the Royal Society, *viz.* that inflammable air is pure phlogiston in the form of air, at least with the element of *heat*; and that fixed air consists of dephlogisticated and inflammable air; both which doctrines had been first advanced by Mr. KIRWAN, before I had made the experiments which I then thought clearly proved them.

Such were the hypotheses to which I had a view, when I began the following course of experiments, which I hope will be an admonition to myself, as well as to others, to adhere as rigorously as possible to *actual observations*, and to be extremely careful not to overlook any circumstance that may possibly contribute to any particular result. I shall have occasion to notice my own mistakes with respect to *conclusions*, though all the *facts* were strictly as I have represented them.

them. But whilst philosophers are faithful narrators of what they observe, no person can justly complain of being misled by them; for to *reason* from the facts with which they are supplied, is no more the province of the person who discovers them, than of him to whom they are discovered.

One of the most simple of all phlogistic processes, is that in which metals are ignited in dephlogisticated air. I therefore began with this, with a view to ascertain whether any *water* be produced when the air is made to disappear in it. Accordingly, into a glass vessel containing seven ounce measures of pretty pure dephlogisticated air, I introduced a quantity of iron turnings (which is iron in small thin pieces, exceedingly convenient for these and many other experiments) having previously made them, together with the vessel, the air, and the mercury by which it was confined as dry as I possibly could. Also, to prevent the air from imbibing any moisture, I received it immediately in the vessel in which the experiment was made, from the process of
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of procuring it from red precipitate, so that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and presently reduced the 7 ounce measures of air to .65 of a measure; but I found no more water after this process than I imagined it had not been possible for me to exclude, as it bore no proportion to the air which had disappeared. Examining the residuum of the air, I found one-fifth of it to be fixed air, and when I tried the purity of that which remained by the test of nitrous air, it did not appear that any phlogisticated air had been produced in the process; for though it was more impure than I suppose the air with which I began the experiment must have been, it was not more so than the phlogisticated air of the 7 ounce measures, which had not been affected by the process, and which must have been contained in the residuum, would necessarily make it. In this case one measure of this residuum and two of nitrous air occupied the space of .32 of a measure.

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In another experiment of this kind, ten ounce measures of dephlogisticated air were reduced to .8 of a measure, and by washing in lime water to .38. In another experiment, in which $7\frac{1}{2}$ ounce measures of dephlogisticated air were reduced to half an ounce measure, of which one-fifth was fixed air, the residuum was quite as pure as the air with which I began the experiment, the test with nitrous air, in the proportions above-mentioned, giving .4 in both cases. To what circumstance the difference might be owing I cannot tell.

In these experiments the fixed air must, I presume, have been formed by the union of the phlogiston from the iron and the dephlogisticated air in which it was ignited; but the quantity of it was very small in proportion to the air which had disappeared, and at that time I had no suspicion that the iron, which had been melted, and gathered into round balls, could have imbibed it; a melting heat having been sufficient, as I had imagined, to expel every thing that was capable of assuming the form of air from any substance

substance whatever. I was therefore entirely at a loss about what must have become of the air.

Sensible, however, that such a quantity of air must have been imbibed by *something*, to which it must have given a very perceivable addition of weight, and seeing nothing else that could have imbibed it, it occurred to me to weigh the calx into which the iron had been reduced; and I presently found, that the dephlogisticated air had actually been imbibed by the melted iron, in the same manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however improbable such an absorption might have appeared to me *a priori*. In the first instance, about twelve ounce measures of dephlogisticated air had disappeared, and the iron had gained six grains in weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the same manner, gained similar additions of weight, which was always very nearly that of the air which had disappeared.

This

This calx of iron, I then concluded, was by no means what I had before taken it to be, viz. a *pure calx*, or *slag*, but either the calx, or the iron itself, saturated with pure air. This calciform substance I found, by various experiments, to be the same thing with the *scales* that fly from iron when it is made red-hot, or the substance into which it runs in a very intense heat, in an open fire*.

Concluding from the preceding experiment, that iron sufficiently heated, was capable of saturating itself with pure air, extracted from the mass of the atmosphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I presently found, that perfect iron was easily fused in this way, and continued in this fusion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was on the contrary *imbibing* air; and when it was saturated the fusion ceased, and the heat of my lens could not make any farther impres-

* It is also the same thing with *finery cinder*, of which an account will be given hereafter.

fion

fion upon it. When this was the case, I always found that it had gained weight in the proportion of $7\frac{1}{2}$ to 24, which is very nearly *one-third* of its original weight. The same was the effect when I melted *steel* in the same circumstances, and also every kind of iron on which the experiment could be tried. But I have some reason to think, that with a greater degree of heat than I could apply, the iron might have been kept in a state of fusion somewhat longer, and by that means have imbibed more air, even more than one-third of its original weight.

There was a peculiar circumstance attending the melting of *cast iron* with a burning lens, which made it impossible to ascertain the addition that was made to its weight, and at the same time afforded an amusing spectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began to disperse in a thousand directions, exhibiting the appearance of a most beautiful fire-work, some of the particles flying to the distance of half a yard from the place of fusion; and the whole was attended

with a considerable hissing noise. Some of the largest pieces which had been dispersed in this manner I was able to collect, and having subjected them to the heat of the lens, they exhibited the same appearance as the larger mass from which they had been scattered.

When I melted this cast iron in the bottom of a deep glass receiver, in order to collect all the particles that were dispersed, they firmly adhered to the glass, melting it superficially, though without making it crack, so that it was still impossible to collect and weigh the particles. However, I generally found that, notwithstanding the copious dispersion, what remained after the experiment rather exceeded than fell short of the original weight of the iron. Sometimes a piece of common iron, and especially steel, would make a little hissing in the fusion, and a particle or two would fly off; but this was never considerable*.

* On being informed of the above-mentioned phenomena, Mr. WATT concluded, that the basis of the dephlogisticated air united to the phlogiston of the iron, and formed

Having now procured what I thought to be a new calx of iron, or a calx saturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the same manner that I had before made iron, and various other metals, by melting them in a vessel containing inflammable air. In this I succeeded; but in the course of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had saturated with pure air, and putting it into a glass vessel containing inflammable air, confined by water, threw upon it the focus of the lens, and presently perceived the inflammable air to disappear, and without thinking of any thing escaping from the calx of iron (which had been subjected to a greater heat before) I imagined that I should have found the addition of the weight of air in the iron, and the result might be an iron different from the formed *water*, which was attracted by, and remained so firmly united to the calx of iron, as to resist the effects of heat to separate them.

common

common sort. But I found, to my surprise, that the iron which had exhibited no new appearance in this mode of treatment, had lost weight, instead of gaining any. The piece of iron on which I made this first experiment weighed $11\frac{1}{2}$ grains, and $7\frac{1}{2}$ ounce measures of inflammable air had disappeared while the iron had lost $2\frac{1}{2}$ grains.

Considering the quantity of inflammable air that had disappeared, viz. $7\frac{1}{2}$ ounce measures, and the dephlogisticated air which had been expelled from the iron, viz. $2\frac{1}{2}$ grains, which is equal to about 4.1 ounce measures, I found that they were very nearly in the proper proportion to saturate each other, when decomposed by the electric spark, viz. two measures of inflammable air to one of dephlogisticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water in the receiver in which the experiment was made, and having neglected to examine the state of the air that remained, except in a general way, by which

I found, that it was still, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining inflammable and dephlogisticated air, I repeated the experiment in a vessel in which the inflammable air was confined by mercury, and both the vessel and the mercury had been previously made as dry as possible. I had no sooner begun to heat the iron, or rather *slag*, in these circumstances, than I perceived the air to diminish, and at the same time the inside of the vessel to grow very cloudy, with particles of dew that covered almost the whole of it. These particles by degrees gathered into drops, and ran down the sides of the vessel in all places, except where it was heated by the sun-beams; so that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let loose from the iron in this mode of operation; though I was afterwards taught by Mr. WATT to correct this hypothesis, and to account for this

this result in a different manner. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment, by means of a piece of filtering paper, carefully introduced to absorb it, I found it to be, as nearly as possible, of the same weight with that which had been lost by the iron: and also, in every experiment of this kind, in which I attended to this circumstance, I found that the quantity of inflammable air which had disappeared, was about double to that of the dephlogisticated air set loose from the iron, supposing that weight to have been reduced into air. Thus at one time I made a piece of this slag imbibe $5\frac{1}{2}$ ounce measures of inflammable air, while it lost as much as the weight of about 3 ounce measures of dephlogisticated air, and the water collected weighed 2 grains. Another time a piece of slag lost 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I shall only mention one more experiment of

this kind, in which 64 ounce measures of inflammable air were reduced to .92 of an ounce measure, and the iron had lost 2 grains, equal in weight to 3.3 ounce measures of dephlogisticated air. In all the above-mentioned experiments, the inflammable air was that which is produced by the solution of iron in acids.

As before I had finished this course of experiments I had satisfied myself that inflammable air always contains a portion of water, and also, that when it has been some time confined by water, it imbibes more, so as to be increased in its specific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a vessel of dry mercury from the vessel in which it was generated; but I presently perceived that water was produced in this case also, and to appearance as copiously as in the former experiment. Indeed, the quantity of water produced, which so greatly exceeded the weight of all the inflammable air, is sufficient to prove that it must have had

had some other source than any constituent part of that air, or the whole of it, together with the water contained in it, without taking into consideration the corresponding loss of weight in the iron.

I must here observe, that the iron slag which I had treated in this manner, and which had thereby lost the weight which it had acquired by melting in dephlogisticated air, became *perfect iron* as at first, and was then capable of being melted by the burning lens again; so that the same piece of iron would serve for these experiments as long as the operator should chuse. It was evident, therefore, that if the iron had lost its phlogiston in the preceding fusion, it had acquired it again from the inflammable air which it had absorbed; and I do not see how the experiment can be accounted for in any other way, which necessarily implies the reality of phlogiston as a constituent principle in bodies. This, at least, is the most natural way of accounting for the appearances.

Having had this success with the calx, or scales of iron, I tried the calx of copper,

or those scales which fly from it when it is made red-hot; and I found water produced in the inflammable air in the same manner as when I used the scales of iron in the same circumstances. I also had the same result when I revived *precipitate per se* in inflammable air; but having at that time a very weak winter's sun, I could not make the experiment with so much advantage as I could have wished*.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the sun-beams, if it were possible for it to attract air, or whatever else it is that is the immediate cause of its additional weight. Three ounces of common iron filings, exposed to a strong heat in an earthen retort, gained

* It will be seen that when I repeated this experiment to more advantage afterwards, I found no more water than might be supposed to have been contained, as an extraneous substance, either in the inflammable air, or the red precipitate; which makes a striking contrast with the experiment in which *iron* is used, and is greatly in favour of the doctrine of phlogiston.

11 dwts.

11 dwts. or 264 grains, and yet was very far from having been completely fused. Having a glass tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water ascended within the tube; which shews that the iron was then in a state of absorbing, and not of giving out any air.

Seeing so much water produced in these experiments with inflammable air, I was particularly led to reflect on the relation which they bore to each other, and especially Mr. CAVENDISH's ideas on the subject. He had told me that, notwithstanding the experiments of which I had given an account to the Royal Society, and from which I had concluded that inflammable air was pure phlogiston, he was persuaded that water was essential to the production of it, and even entered into it as a constituent principle. At that time I did not perceive the force of the arguments which he stated to me, especially as, in the experiments with charcoal, I totally dis-

perfed any quantity of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no fufpicion that the wet leather on which my receiver flood could have any influence in the cafe, while the piece of charcoal was fubject to the intense heat of the lens, and placed feveral inches above the leather. I had alfo procured inflammable air from charcoal in a glazed earthen retort two whole days fucceffively, in which it had given inflammable air without intermiffion.

Alfo iron filings in a gun-barrel, and a gun-barrel itfelf, had always given inflammable air whenever I tried the experiment.

These circumftances, however, deceived me, and perhaps would have deceived any other perfon; for I did not know, and could not have believed, the powerful attraction that charcoal, or iron, appear to have for water when they are intenfely hot. They will find, and attract it, in the midft of the hotteft fire, and through any pores that may be left open in a retort; and iron filings are feldom fo dry as not to have moifture enough adhering

adhering to them, capable of enabling them to give a considerable quantity of inflammable air. But my attention being now fully awake to the subject, I presently found that the circumstances above-mentioned had actually misled me; I mean with respect to the *conclusion* which I drew from the experiments, and not with respect to the experiments themselves, every one of which, I doubt not, will be found to answer, whenever they are tried by persons of sufficient skill and properly attentive to all the circumstances.

Being thus apprized of the influence of unperceived moisture in the production of inflammable air, and willing to ascertain it to my perfect satisfaction, I began with filling a gun-barrel with iron filings in their common state, without taking any particular precaution to dry them, and I found that they gave air as they had been used to do, and continued to do so many hours: I even got ten ounce measures of inflammable air from two ounces of iron filings in a coated glass retort. At length, however,

however, the production of inflammable air from the gun-barrel ceased; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston.

I then repeated the experiment with the charcoal, making the receiver, the stand on which I placed the charcoal, and the charcoal itself, as dry and as hot as possible, and using cement instead of a wet leather to exclude the air. In these circumstances I was not able, with the advantage of a good sun, and an excellent burning lens, to decompose quite so much as two grains of the piece of charcoal, which gave me ten ounce measures of inflammable air; and this, I imagine, was effected by means of so much moisture as was deposited from the air in its state of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as necessary as to that from iron.

It

It was in this state of my experiments that I received an authentic account of those of M. LAVOISIER, on transmitting water through an hot iron tube, and also through a hot copper tube containing charcoal, and thereby procuring large quantities of inflammable air, M. LAVOISIER himself having been so obliging as to send me a copy of his Memoir on that subject. I had heard an account of the experiments some months before; but it was so imperfect a one, that I own I paid little attention to them. At this time, however, I was prepared to be sufficiently sensible of their value.

In my last communications to the Royal Society, it will be seen that I had transmitted the vapour of several fluid substances through red-hot *earthen tubes*, and thereby procured different kinds of air. M. LAVOISIER adopted the same process, but used an *iron tube*; and by means of that circumstance made a very valuable discovery which had escaped me. I had indeed on one occasion made use of an iron tube, and transmitted

mitted steam through it; but not having at that time any view to the production of air, I did not collect it at all, contenting myself with observing that water, after being made red-hot, was still water, there being no change in its sensible properties. Being now farther instructed by the experiment of M. LAVOISIER, I was determined to repeat the process with all the attention I could give to it; but I should not have done this with so much advantage, if I had not had the assistance of Mr. WATT, who always thought that M. LAVOISIER's experiments by no means favoured the conclusion that he drew from them. As to myself, I was a long time of opinion that his conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process. But though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. WATT's observations threw upon them, satisfied me at length that the inflammable air

I hope

air came principally from the charcoal, or the iron.

I shall first relate the result of the experiment that was made with charcoal, and then those with iron and other substances, in contact with which (when they were in a state of fusion, or at least red-hot) I made steam, or the vapour of other liquid substances, to pass. I shall only observe that, previous to this, I began to make the experiments with coated glass tubes, which I found to answer very well during the process, though they never failed to break in cooling. At length I procured a tube of copper, on which, as M. LAVOISIER discovered, steam had no effect; and at last I made use of earthen tubes, with which Mr. WEDGWOOD, that most generous promoter of science, liberally supplied me for the purpose; and these, glazed on the outside only, I find far preferable to copper. They are, indeed, every thing that I could wish for in experiments of this kind; the reason of which will appear in my account of another course of experiments, which I

I hope

I hope to lay before the Society in due time*

The disposition of the apparatus, with which these experiments were made, was as follows: The water was made to boil in a glass retort, which communicated with the copper or earthen tube that contained the charcoal or iron, &c. and which, being placed in an horizontal position, was surrounded with hot coals. The end of this tube opposite to the retort communicated with the pipe of a common *worm tub*, such as is generally used in distillations, by means of which all the superfluous steam was condensed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm-tub, was transmitted into a trough of water, where proper vessels were placed to receive it, and ascertain the quantity of it; after which I could examine the quality of it at leisure†.

* An account of them is contained in this volume, not being thought of sufficient importance to lay before the Royal Society.

† The disposition of this apparatus may be seen Fig. 2.

In the experiment with charcoal, I found unexpected difficulties, and considerable variations in the result; the proportion between the charcoal and water expended, and also between each of them and the air produced, not being so nearly the same as I imagined they would have been. Also the quantity of fixed air that was mixed with the inflammable air varied very much. This last circumstance, however, some of my experiments may serve to explain. Whenever I had no more water than was sufficient for the production of the air, there was never any sensible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the case when I produced the air by means of a burning lens in an exhausted receiver, and also in an earthen retort with the application of an intense heat. I therefore presume, that when the steam transmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwise have been. The

extremes that I have observed in the proportion of the fixed to the inflammable air have been from one-twelfth to one-fifth of the whole. As I generally produced this air, the latter was the usual proportion; and this was exclusive of the fixed air that was intimately combined with the inflammable air, and which could not be separated from it except by decomposition with dephlogisticated air; and this combined fixed air I sometimes found to be one-third of the whole mass, though at other times not quite so much.

To ascertain this, I mixed one measure of this inflammable air from charcoal (after the uncombined fixed air had been separated from it by lime-water) with one measure of dephlogisticated air, and then fired them by the electric spark. After this I always found that the air which remained made lime-water very turbid, and the proportion in which it was now diminished, by washing in lime-water, shewed the quantity of fixed air that had been combined with the inflammable. That the
fixed,

fixed air is not generated in this process, is evident from there being no fixed air found after the explosion of dephlogisticated air and inflammable air from iron*. A whole the

Notwithstanding the above-mentioned variations, the loss of weight in the charcoal was always much exceeded by the weight of the water expended, which was generally more than double that of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever deposited from it.

The experiment which, upon the whole, gave me the most satisfaction, and the particulars of which I shall therefore recite, was

* When I wrote this paper, I imagined that the fixed air, which was found on the decomposition of this inflammable air with dephlogisticated air, had been contained in the inflammable air. But it will appear, in the course of this volume, that it must have been formed by the union of phlogiston (or inflammable air) and dephlogisticated air, made by the explosion; though it is remarkable that no fixed air is formed when the inflammable air from iron is used.

the following. Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very strong heat, so as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce measures of air, one-fifth of which was fixed air, and of the inflammable part nearly one-third more appeared to be fixed air by decomposition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have ascertained the quantity of it) consisting of fixed and inflammable air together, I found some variations in its specific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of 14 grains to 40 ounce measures is pretty near the truth, when the proportion of fixed air is about one-fifth of the whole. With respect to the weight of the inflammable air after the fixed air was separated from it, I found no great difference, and think it may be estimated at 8 grains to 30 ounce measures.

Upon

Upon these principles, the whole weight of the 840 ounce measures of air will be - - - - - 294 grains

that of the charcoal will be 94

that of the water - - - 240

334 which, considering the nature of the experiment, will perhaps be thought to be tolerably near to that of the air.

If the air be analyzed, the 840 ounce measures will be found to contain

168 of uncombined fixed air = 151 grains.

and 672 impure inflammable = 179

so that the whole 840 will weigh - - - 330

It may, however, be safely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itself may be very well supposed to have supplied.

There is, therefore, no reason for deserting the old established hypothesis of *phlogiston* on account of these experiments, since the

fact is by no means inconsistent with it. The pure inflammable air, with the water necessarily contained in it, would weigh no more than about 30 grains, while the loss of weight in the charcoal was 94 grains. But to this must be added the phlogiston contained in 392 ounce measures of fixed air, which, according to Mr. KIRWAN's proportion, will be nearly 65 grains, and this and the 30 grains will be 95 grains.

The basis to this fixed air, as well as to the inflammable, must have been furnished by the *water*; and from this it may be concluded, that the water must have been so far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be considered that, according to the latest discoveries, fixed air and water appear to consist of the same ingredients, namely, dephlogisticated and inflammable air. However, in this change of the water we cannot be absolutely sure that the same proportion of the ingredients is contained, and therefore it cannot be absolutely determined whether the inflammable air
which

which it contains enters wholly into the fixed air, or not. Farther experiments, or a careful comparison of these with those made by Mr. KIRWAN and others, may perhaps throw some light upon this subject. Whether the combined fixed air comes wholly from the charcoal, or whether the charcoal only supplies the phlogiston, and the water its basis, that is, the dephlogisticated air, deserves to be investigated.

Before I conclude my account of the experiments with charcoal, I would observe, that there is another on which I place some dependence, in which, with the loss of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce measures of air, of which the last portion (for I did not examine the rest) contained one-sixth part of uncombined fixed air. This was made in an earthen tube glazed on the outside.

The experiments with *iron* were more satisfactory than those with charcoal, being subject to less variation; and they by no means require us to suppose that the inflammable

air comes from the *water*, but only from the *iron*, as the quantity of water expended, deducting the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this process is between one-third and one-half more than can be procured from iron by a solution in acids, the reason may be, that much phlogiston is retained in the solutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would further observe, that the produce of air, and also the addition of weight gained by the iron, are much more easily ascertained in these experiments than the quantity of water expended in them, on account of the great length of the vessels used in the process, and the different quantities that may perhaps be retained in the worm of the tub; though I did not fail to use all the precautions that I could think of, to guard against any variation on these accounts.

Of

Of the many experiments which I made with *iron*, I shall content myself with reciting the following results. With the addition of 267 grains to a quantity of iron, and the loss of 336 grains of water, I procured 840 ounce measures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the consumption of 254 grains of water, I got 420 ounce measures of air*.

The inflammable air produced in this manner is of the lightest kind, and free from that very *offensive smell* which is generally occasioned by the rapid solution of metals in oil of vitriol, and it is extricated in as little time in this way as it is possible to do it by any mode of solution. On this account it occurred to me, that it must be by much the

* If the perfect accuracy of the former of these experiments may be depended on (and it may always be presumed, that those in which *little water* is expended are preferable to those in which *more* is consumed) the water that necessarily enters into this kind of inflammable air is about equal in weight to the *phlogiston* that is in it. But I propose to give more particular attention to this subject:

cheapest method that has yet been used of filling *balloons* with the lightest inflammable air. For this purpose it will be proper to make use of cast-iron cylinders of a considerable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itself will contribute to the production of air; and therefore may become unfit for the purpose in time; yet, for any thing that I know to the contrary, the same tube may serve for a very great number of processes, and perhaps the change made in the inside surface may protect it from any farther action of the water, if the tube be of sufficient thickness; but this can only be determined by experiment.

Some estimate of what may be expected from this method of procuring inflammable air may be formed from the following observations. About twelve inches in length of a copper tube, three-fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpose than *iron filings*, as they do not lie so close, but admit the steam to pass through their interstices)

stices) when it was heated, and a sufficient quantity of steam passed through it, yielded thirty ounce measures of air in fifty seconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the same manner, gave two hundred ounce measures in one minute and twenty-five seconds; so that this larger tube gave air in proportion to its solid contents compared with the smaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates so readily to some distance, the rate of giving air will always be in a greater proportion than that of the simple diameter of the tube.

The following experiment was made with a view to ascertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when dissolved in acids, will yield about 800 ounce measures of air; but treated in this manner it yielded 1054 ounce measures, and then the iron had gained 329 grains in weight, which

is

is little short of one-third of the weight of the iron.

Considering how little this inflammable air weighs, *viz.* the whole 1054 ounce measures not more than 63 grains, and the difficulty of ascertaining the loss of water to so small a quantity as this, it is not possible to determine, from a process of this kind, how much water enters into the composition of the inflammable air of metals. It would be more easy to determine this circumstance with respect to the inflammable air of charcoal, especially by means of the experiment made with a burning lens *in vacuo*. In this method two grains of charcoal gave at a medium thirteen ounce measures of inflammable air, which, in the proportion of 30 ounce measures to 8 grains, will weigh 3.3 grains; so that water in the composition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be some difficulty with respect to the fixed air intimately combined with this kind of inflammable air.

Since

Since iron gains the same addition of weight by melting in *dephlogisticated air*, and also by the addition of *water* when red-hot, and becomes, as I have already observed, in all respects the same substance, it is evident, that this *air* or *water*, as existing in the iron, is the very same thing; and this can hardly be explained but upon the supposition that *water* consists of two kinds of air, *viz.* inflammable and dephlogisticated. I shall endeavour to explain these processes in the following manner.

When iron is melted in dephlogisticated air, we may suppose that, though part of its phlogiston escapes, to enter into the composition of the small quantity of fixed air which is then procured, yet enough remains to form *water* with the addition of the dephlogisticated air which it has imbibed, so that this *calx* of iron consists of the intimate union of the pure *earth of iron* and of *water*; and therefore when the same *calx*, thus saturated with water, is exposed to heat in inflammable air, this air enters into it, destroys the attraction between the water and the earth,

earth, and revives the iron, while the water is expelled in its proper form.

Consequently, in the process with *steam*, nothing is necessary to be supposed but the entrance of the water, and the expulsion of the phlogiston belonging to the iron, no more phlogiston remaining in it than what the water brought along with it, and which is retained as a constituent part of the water, or of the new compound.

Having procured water from the scales of iron (which I must again observe is, in all respects, the same substance with iron melted in dephlogisticated air, or saturated with steam by means of heat) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I should be able to produce the same effect by heating it with charcoal in a retort; and I had likewise no doubt but I should be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of these conjectures I was right; but with respect to the latter, I was totally mistaken.

Having

Having made the scales of iron, and also the powder of charcoal very hot, previous to the experiment, so that I was satisfied that no air could be extracted from either of them separately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in which I could give it a very strong heat; and connected with it proper vessels to condense and collect the water which I expected to receive in the course of the process. But, to my great surprise, not one particle of *moisture* came over, but a prodigious quantity of *air*, and the rapidity of its production astonished me; so that I had no doubt but that the weight of the air would have been equal to the loss of weight both in the scales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air, and the inflammable air, which remained when the fixed air was separated from it, was of a very remarkable kind, being quite

as

as heavy as common air. The reason of this was sufficiently apparent when it was decomposed by means of dephlogisticated air; for the greatest part of it was fixed air.

The theory of this process I imagined to be, that the phlogiston from the charcoal reviving the iron, the water with which it had been saturated, being now set loose, affected the hot charcoal as it would have done if it had been applied to it in the form of *steam* as in the preceding experiments; and therefore the air produced in these two different modes have a near resemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the cases I found these proportions subject to variations. in one process with charcoal and scales of iron, the first produce contained one-fifth of uncombined fixed air, the middle part one-tenth, and the last none at all. But in all these cases the proportion of combined fixed air varied very little.

Why *air* and not *water* should be produced in this case, as well as in the preceding,
ing,

ing, when the iron is equally revived in both, I do not pretend perfectly to understand. There is, indeed, an obvious difference in the circumstances of the two experiments; as in that with charcoal the phlogiston is found in a combined state; whereas in that of inflammable air, it is loose, or only united to water; and perhaps future experiments may discover the operation of this circumstance*.

* This experiment seems to be decisive against the hypothesis of Mr. LAVOISIER, and others, who say that the inflammable air procured by means of iron and charcoal comes from the water, and who think that by this means they can exclude phlogiston. For, according to them, neither the scales of iron, nor the charcoal, contain phlogiston, or any thing from which inflammable air can be made, but are merely substances capable of imbibing pure air, and thereby setting at liberty the inflammable air contained in the water. As to the scales of iron, they were only iron saturated with dephlogisticated air. But had this been the case, there was nothing in either of the materials made use of in this experiment from which the inflammable air could possibly come, there being no water contained in either of them. But supposing the reality of phlogiston, and its constituting a part of metals, of charcoal, and of inflammable air, the experiment is very intelligible.

There is some analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itself imbibing dephlogisticated air. In the former case *water* is produced, and in the latter *fixed air*. However, this case of iron imbibing dephlogisticated air, more nearly resembles the case of the *blood in the lungs* imbibing the same kind of air, and in both the cases as dephlogisticated air is imbibed, fixed air is formed. This, therefore, seems to be a confirmation of the conclusion which I drew from my former experiments on blood, *viz.* that it parts with phlogiston in respiration. Only I would now add, that at the same time that it parts with phlogiston it takes in dephlogisticated air, which makes the case perfectly similar to that of the experiment with *iron*, which likewise parts with phlogiston to form fixed air, at the same time that it imbibes dephlogisticated air in contact with which it is fused.

I propose to reserve for a future communication the continuation of these experiments, containing an account of the application

cation of the same process to other substances; but it may not be amiss just to mention a few of the *general results*, and those which have the nearest connexion with the experiments recited above.

After having transmitted steam in contact with *charcoal* and *iron* in a copper tube, I proposed to do the same with other substances containing phlogiston, and I began with *bones*, which were burnt black, and had been subjected to an intense heat, covered with sand, in an earthen retort. From three ounces of bones thus prepared, and treated as I had done the charcoal, I got 840 ounce measures of air, with the loss of 288 grains of water. The bones were by this means made perfectly white, and had lost 110 grains of their weight. As the air ceased to come a considerable time before all the water had been transmitted through the tube containing them, I concluded that the air was formed from the phlogiston contained in the bones, and so much water as was necessary to give it the form of air.

This air differs considerably from any other kind of inflammable air, being in several respects a medium between that from charcoal and that from iron. It contains about one-fourth of its bulk of uncombined fixed air, but not quite one-tenth intimately combined with the remainder. The water that came over was blue, and pretty strongly alkaline, which must have been occasioned by the volatile alkali not having been entirely expelled from the bones in the former process, and its having in part dissolved the copper of the tube in which the experiment was made.

I subjected to the same process a variety of substances that are said not to contain phlogiston, but I was never able to procure inflammable air by means of them; which strengthens the hypothesis of the principal element in the constitution of this air having been derived from the substance supposed to contain phlogiston, and therefore that phlogiston is a real substance capable of assuming the form of air by means of water and heat.

The

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the same result when I made use of small nails of *cast iron*, except that these were firmly fastened together after the experiment, the surfaces of them being crystallized, and the crystals mixing with each other, so that it was with great difficulty that they could be got out of the tube after the experiment; and in general the solid parts of the nails were broken before they were separated from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means so firmly.

Cast iron annealed (by being kept red-hot in charcoal) is remarkably different from the cast iron which has not undergone that operation, especially in its being, to an extraordinary degree, more soluble in acids. With the turnings of annealed cast iron I made the following experiment. From 960 grains of this iron, and with the loss of 480 grains of water, I got 870 ounce measures of inflammable air, and transmitting steam

I 2 through

through them a second time, I got 150 ounce measures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were easily broken and got out of the tube, whereas it had required a long time, and a sharp steel instrument, to clear the tube of the cast-iron nails.

Willing to try the effect of heating iron, and other substances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogisticated air, the air was diminished as rapidly, and the inside of the vessel was covered with a *black footy matter*, which when exposed to heat, readily sublimed in the form of a white vapour, and left the glass quite clean. The iron, after the experiment, was quite brittle, and must, I presume, be the same thing with iron that is *sulphurated*; but I did not particularly examine it. Of seven ounce measures of vitriolic acid air, in one of these experiments, not more than three-tenths of an ounce measure remained; of this two-thirds

thirds was fixed air, and the residuum of this was inflammable. I had put three of such residuums together, in order to make the experiment with the greater certainty.

Having transmitted *steam*, or the vapour of water, through a copper tube, I was willing to try the effects of *spirit of wine* through the same tube when red-hot, having before procured inflammable air by sending the same vapour through a red-hot tobacco-pipe. In this case, the vapour of the spirit of wine had no sooner entered the hot copper tube, than I was perfectly astonished at the rapid production of air. It resembled the blowing of a pair of bellows. But I had not used four ounces of the spirit of wine before I very unexpectedly found, that the tube was perforated in several places; and presently afterwards it was so far destroyed, that in attempting to remove it from the fire it actually fell in pieces. The inside was full of a black sooty matter resembling lamp-black.

Upon this I had recourse to *earthen tubes*, and found, that by melting copper and other metals in them, and transmitting the

vapour of spirit of wine in contact with them, different substances were formed according to the metals employed. The new substances hereby formed may be said to be the several metals super-saturated with phlogiston, and may perhaps not be improperly called the *charcoal of the metals*.

That this appellation is not very improper, may appear from these substances yielding inflammable air very copiously when they are made red-hot, and the steam of *water* is transmitted in contact with them, just as when the charcoal of wood is treated in the same manner. The detail of these experiments I reserve for another communication, as also those of the conversion of *spirit of wine, æther, and oil*, into different kinds of inflammable air, by transmitting them, in vapour, through hot earthen tubes. In the mean time, I shall think myself happy if the communication of the preceding experiments shall give any satisfaction to the Members of the Society*.

* An account of these experiments will be found in the present volume. They were not completed soon enough to send to the Society before the publication of this volume.

P O S T S C R I P T.

BEFORE I close this paper, I wish to make a few *general inferences* from the principal of the experiments above-mentioned, especially relating to the proportional quantity of phlogiston contained in *iron and water*.

When any quantity of iron is melted in dephlogisticated air, it imbibes the greatest part of it, and gains an addition of weight very nearly equal to that of the air imbibed. Thus the absorption of twelve ounce measures of dephlogisticated air gave an addition of six grains to the piece of iron which had been melted in it. But there was always a quantity of *fixed air* produced in this process; and on the supposition that this air consists of the union of dephlogisticated and inflammable air, it proves that the dephlogisticated air which enters the iron expels

more phlogiston than is necessary to constitute an equal weight of water, so that *water* does not contain so much phlogiston as *iron*; but the difference is not very considerable.

Admitting Mr. KIRWAN's conclusion, *viz.* that 100 cubic inches of fixed air contain 8,357 grains of phlogiston, the .13 of an ounce measure of fixed air, which (in an experiment recited in these papers) was found in the residuum of seven ounce measures of dephlogisticated air absorbed by iron, would not have contained more than .01 of a grain of phlogiston, or about .16 of an ounce measure of inflammable air. Then, as the absorption of 12 ounce measures of dephlogisticated air occasioned an addition of 6 grains to the weight of the iron which had absorbed it, the absorption of seven ounce measures must have occasioned the addition of 3.5 grains to the iron which had imbibed it. But the same addition of weight to iron given by *steam* (which carries its own inflammable air along with it) would have expelled near 12 ounce measures of inflammable air: consequently, about 10 ounce measures

tures of inflammable air (or the phlogiston requisite to form it) must, in the former experiment, have been retained in the iron, in order to compose the *water* which was now made by the union of the dephlogisticated air imbibed by the iron and the phlogiston contained in it: and therefore the proportion between the quantity of phlogiston in *iron* to that which is contained in an equal weight of water, may be about 12 to 10, or more accurately to 10.4.

Had no fixed air at all been found in the residuum above-mentioned, it might have been concluded, that water had contained the very same proportion of phlogiston with iron. Since when iron that has been saturated with dephlogisticated air is heated in inflammable air (in which process an equal weight of water is produced, and the loss of weight in the iron is equal to that of such a quantity of dephlogisticated air as would have been one-half of the bulk of the inflammable air which disappears in that process) it might have been concluded, that one-

one-fifth of any quantity of water had been inflammable air.

For, neglecting the difference between the weight of dephlogisticated and common air, which is not considerable, and estimating the latter at $\frac{1}{5}$ th part that of water, and inflammable air at one-tenth of the weight of common air, an ounce measure of dephlogisticated air will weigh .6 of a grain, and two ounce measures of inflammable air will weigh .12 of a grain, which numbers are to each other as 5 to 1*.

* It appears from the prosecution of these experiments, that the water which is found on heating the scales of iron in inflammable air, is not formed by the dephlogisticated air expelled from them uniting with the inflammable air in the vessel, but was the water previously contained in the scales, which is made to quit its place by the introduction of the phlogiston from the inflammable air; yet that water carries out with it not much less phlogiston than was taken in by the iron, and a little more must be allowed for that water which was necessary to make inflammable air, and which could not enter the iron when it was revived; so that, on the whole, the phlogiston in the water that is found after the process must be very nearly the same quantity that is imbibed by the iron, and the

Though, in consequence of the small quantity of fixed air which is found in the process of melting iron in dephlogisticated air, this conclusion is not accurate, it is pretty nearly so; and it is remarkable that, upon this supposition, about as much inflammable air is expelled from iron when water is combined with it, as the water itself brings along with it, as an essential ingredient in its composition. For in one experiment 296 grains added to the weight of a quantity of iron by steam, made it to yield about 1000 ounce measures of inflammable air. This would weigh 60 grains, and one-fifth of the 296 grains of water will be 59.2 grains. Again, 267 grains added to iron by steam made it to yield 840 ounce measures of inflammable air, which would weigh 50.4 grains, and one-fifth of the 267 would be 53.4 grains.

the water is nearly the same that would have been produced, on the supposition of its being made from dephlogisticated air expelled from the scales uniting with the inflammable air in the vessel.

When

When the experiments on the melting of iron in dephlogisticated air shall be repeated on a larger scale, which it will not be difficult to do by the help of a larger burning lens than I am at present possessed of, it will be easy to reduce these calculations to a greater certainty. All that I can do at present is to approximate to such general conclusions as I have mentioned; but they are of so much consequence in philosophy, that it will certainly be well worth while to ascertain them with as much accuracy as possible. Nice calculations would be ill bestowed on the imperfect *data* which I am as yet able to furnish. Attention must also be given to the quantity of water contained in inflammable air from iron; which not being yet ascertained is not considered in these inferences. I wish only to hint in this Postscript, that some important conclusions seem to be nearly within our reach.

S E C-

SECTION IV.

Farther Observations relating to the Composition of Water.

THAT dephlogisticated and inflammable air, at least with the element of *heat*, constitute *water*, seems to be clearly inferred from the experiments in the preceding section. Some difficulties, however, have occurred respecting this theory, from my subsequent experiments, which I shall propose with the same fidelity as I have done those which favour the hypothesis. But as I mean to throw into this section all the observations that I have made upon the subject, I shall first recite some experiments which perfectly agree with the former, and evidently lead to the same conclusion. Some of them are those of which only the general result was given before.

I have

I have observed, that, when that iron which has been melted in the open air (or that which has been altered by the passing of steam over it red-hot) was heated by a burning lens in inflammable air, the air disappeared, and a considerable quantity of water was produced. I had the same result with *scales of copper*. These scales became of a genuine copper colour in this process; so that I had no doubt of their being copper revived. The water was so copious, that when only $3\frac{1}{2}$ ounce measures of air were absorbed, the water formed in drops on the inside of the vessel, and some of them ran down it.

I also procured water when I decomposed dephlogisticated and inflammable air from iron by the electric spark in a close vessel, which is an experiment similar to those that were made by Mr. LAVOISIER, at Paris. I put 3.75 ounce measures of a mixture of air, of which one-third was dephlogisticated, and two-thirds inflammable from iron, in the close vessel; and after the explosion I found in it one grain of moisture. The
dephlo-

dephlogisticated air in this mixture would have weighed a little more than 0.75 of a grain. But repeating this experiment with half as much dephlogisticated as inflammable air, I could not perceive any water after the experiment. Neither was there any fixed air formed in it. Again, using inflammable air from wood, in the same proportion from 3.8 ounce measures of the mixture, I got 0.8 of a measure of fixed air, which was actually absorbed by lime-water. Some water also was evidently produced.

Using more precautions to exclude all water from either of the two kinds of air before the experiment (both the dephlogisticated air, which was from nitre, and the inflammable air, which was from charcoal, being from the first received in mercury, and always confined by it) I still found a little water after the explosion.

I varied this experiment by producing the inflammable air in the dephlogisticated air as follows. Into a vessel containing dephlogisticated air confined by mercury, I introduced a piece of perfect charcoal, as hot

from the fire as I could bear to handle it, and threw upon it the focus of the lens, so that a quantity of the air was imbibed; but I could not perceive that any moisture was formed. Afterwards, when I resumed the process, the air which the charcoal had imbibed was expelled again, and very little more was absorbed. From 7 ounce measures there remained 4, of which more than 2 was pure fixed air. No water could be expected after this process. For even had there been a small quantity of moisture in the vessel, it would have been absorbed by the charcoal, and have enabled it to yield inflammable air. The phlogiston the charcoal contained uniting with the dephlogisticated air, free from moisture, formed, I presume, the fixed air that was found after this process.

But the greatest difficulty that occurred with respect to the preceding theory of the constitution of water, arose from my never having been able to procure any water when I revived mercury from red precipitate in inflammable air, or at least not more than

than may be supposed to have been contained in the inflammable air, as an extraneous substance.

In order to make the experiments with the *scales of iron*, and that with the *red precipitate*, as much alike as possible, and that I might compare them to the greatest advantage, I made them immediately one after the other, with every circumstance as nearly as I could the same. The inflammable air was the same in both the experiments, and both the *scales of iron*, and the *red precipitate*, were made as dry as possible. They were heated in vessels of the same size and form, and equally confined by dry mercury. And yet when I heated the former, water was formed as copiously as I have described it before, viz. actually running down the inside of the vessel in drops, though only 4 ounce measures of inflammable air were absorbed. But though I heated the red precipitate till 8 ounce measures of the inflammable air was absorbed, and only three-fourths of an ounce measure of air remained, there was hardly any sensible quantity of

water produced, certainly not one-tenth of what appeared in the experiment with the scales of iron.

There was this difference, however, in the two results. In what remained from the experiment with red precipitate, I at this time perceived a slight appearance of *fixed air*, whereas there was none in what remained from the scales of iron. The residuum also from the red precipitate had in it a very small portion of dephlogisticated air. For being mixed with an equal measure of nitrous air, the standard of it was 1.8. I must also observe that the inflammable air disappeared much more rapidly in the process with red precipitate, than in that with the scales of iron.

Fixed air, however, was no necessary result in this experiment*. For I particu-

* It will be seen in the section relating to inflammable air, that there is a slight appearance of fixed air, in the decomposition of that inflammable air which is produced by means of *spirit of salt*; and, not being aware of this at the time of making these experiments, I took no account how the air was made.

larly observed, that when, upon another occasion, I reduced 8 ounce measures of inflammable air to 0.22 of a measure, I found no more than what I judged to be much less than a grain of water, and without any appearance of fixed air whatever. Again, I reduced 6 ounce measures of inflammable air in this process to 0.42 of a measure, without producing any more water than before, or any fixed air at all. I also had the same result in using the black powder of lead and mercury, instead of red precipitate; reducing 6.5 ounce measures to 0.4 of a measure, without finding any very sensible quantity of water, or any fixed air.

In this experiment there can be no doubt but that the dephlogisticated air dislodged from the red precipitate, united with the inflammable air in the vessel; and as no water equal to the weight of the two kinds of air was produced, they must have formed some more *solid substance*, which, in the small quantities I was obliged to use, could not be found.

It may be clearly inferred from this experiment, that red precipitate is a substance by no means similar to the scales of iron; as the latter appears to contain within itself all the elements of water; and therefore as the inflammable air enters into it, a quantity of water, equal in weight to the loss sustained by the scales in their revival in the form of iron, is found in the recipient. Whereas in the experiment with the red precipitate, there is certainly no more water found, than may be supposed to have been contained in the inflammable air which disappeared, or to have been lodged without being perceived among the particles of the precipitate itself. Consequently the scales of iron must be considered as the calx of iron united to *water*, and red precipitate as mercury, united to *dephlogisticated air*, or rather, perhaps, as Mr. KIRWAN supposes, to *fixed air*, the phlogiston belonging to which revives the mercury, while its other component part, the dephlogisticated air, is set at liberty, forming an union with the element of *heat*.

The

The difficulty with respect to what becomes of the two kinds of air, was not lessened by the attempts which I made to collect all that I could from repeated decompositions of inflammable and dephlogisticated air in a close vessel.

As I had produced water in this process when I made no more than a single explosion at a time, I thought that by continuing to make explosions in the same vessel, the water would not fail to accumulate, till I might collect what quantity I pleased; and I intended to have collected a considerable part of an ounce. And as I should know exactly what quantity of air I decomposed, I had no doubt of being able to ascertain the proportion that the *water* and *air* bore to each other.

With this view I made a mixture of a large quantity of air, one-third dephlogisticated, and two-thirds inflammable from iron and oil of vitriol. But though I had a sensible quantity of water at the first explosion (in each of which was used between four and five ounce measures of the mixture of

air) I was surpris'd to perceive no very sensible increase of the quantity of water on repeating the explosions. Having, therefore, expended 48 ounce measures of the mixture, I discontinued the process; and collecting the water with all the care that I could, I found no more than three grains, when there ought to have been eleven.

In this process the inside of the vessel was always very black after each explosion, and when I poured in the mercury after the explosion, though there was nothing visible in the air within the vessel, there issued from the mouth of it a *dense vapour*. This was even the case, though I waited so much as two minutes after any explosion before I proceeded to put in more mercury in order to make another; which if the vapour had been *steam*, would have been time more than sufficient to permit it to condense into water. I even perceived this vapour when I had a quantity of water in the vessel, and the explosion was consequently made over it, as well as in contact with the sides of the vessel which were wetted with it; so that

that as this vapour had passed through the whole body of water when the vessel was inverted, it is probable that it must have consisted of something else than mere *water*.

But I was never able to collect any quantity of it, though it must have been something produced by the union of the two kinds of air.

In order to collect a quantity of the matter that formed this vapour, I contrived the following apparatus. In a cork (*a*) Fig. 3, with which I could shut the orifice of the strong glass vessel (*b*) in which the explosions were made, I had two perforations. Through one of these (*c*) I poured the mercury, by means of a glass funnel; but into the other was introduced a glass tube, which, being bended, was inserted, by means of a cork, into a thin glass vessel (*d*) and went almost to the bottom of it. A small hole was also made in the cork, to permit the air to go out. Consequently, all the air that remained in the strong glass vessel, with whatever vapour it might contain, must, as I poured in the mercury, necessarily pass

through the glass tube, and be diffused through the thin glass vessel; in which I imagined that all its contents, fluid or solid, must be deposited. However, though I repeated the experiment several times with this apparatus, making about twenty explosions in each, I could not find any deposit in the vessel, besides a small quantity of *water*; which, added to the water collected in the strong vessel, came far short of the weight of the air that was decomposed.

All the conjecture that I can advance, in order to explain this phenomenon, is that, since *soot* yields pure air, as will appear in the course of this volume, part of the soot is formed by the union of the dephlogisticated air in the atmosphere and the inflammable air of the fuel. But *smoke*, which contains much *soot*, is soon dispersed, and becomes invisible in the open air. Such, therefore, may be the case here. The soot formed by the union of the two kinds of air may be diffused through the air, in the vessel in which they are exploded, and be carried invisibly into the common atmosphere, which
may

may account for my not being able to collect any quantity of it in this apparatus.

Hoping to succeed better in collecting this volatile matter by means of a quantity of water incumbent upon the mercury, in the strong glass vessel in which the explosions were made (though I had found that part of it could escape through the water) I decomposed a great quantity of the two kinds of air in these circumstances; and presently found that the water became very cloudy, and was at length full of a blackish matter. This I collected, and found that it remained perfectly black upon the earthen vessel in which the water containing it was evaporated; which would not have been the case if the blackish matter in the water had been that *pewder of mercury*, which is produced by agitating it in pure water. For that black mass always became white running mercury the moment the water was evaporated from it. Could I have collected a sufficient quantity of this black matter, I might have satisfied myself whether it was a proper *soot* or not.

Mr.

Mr. WARLTIRE first observed this cloudiness in a vessel in which he burned inflammable air; but it is remarkable that sometimes I got it repeatedly in these explosions, all the inside of the vessel becoming quite black after the explosion, and at other times I have not been able to get this appearance at all; so that I am not yet able to determine on what it depends. At one time, having the inside of the strong glass tube made very black with these explosions, I let it remain a day or two exposed to the common air, when the blackness disappeared, leaving the inside of the vessel covered with small globules of white mercury. It seems, therefore, that part of the phlogiston of the inflammable air sometimes attaches itself to the vapour of mercury, diffused invisibly through the space within the vessel, and that it quits it to unite to the air of the atmosphere.

That water in great quantities is sometimes produced from burning inflammable and dephlogisticated air is evident from the experiments of Mr. CAVENDISH and Mr.

LAVOISIER.

LAVOISIER. I have also frequently collected considerable quantities of water in this way, though never quite so much as the weight of the two kinds of air decomposed. My apparatus for this purpose was the following. Into the mouth of a large glass balloon (*a*) Fig. 4, I introduced a tube from the orifice of which there continually issued inflammable air, from a vessel containing iron and oil of vitriol. This being lighted, continued to burn like a candle. Presently after the lighting of it, the inside of the balloon always became cloudy, and the moisture soon gathered in drops, and settled in the lower part of the balloon. To catch what might issue in the form of vapour, in the current of air through the balloon, I placed the glass tube (*b*) in which I always found some water condensed. It is very possible, however, that in both these modes of experimenting, the water may be converted into a kind of vapour, which is very different from *steam*, and capable of being conveyed a great way through air, or even water, without condensation, along with the air with which it is mixed; and on this account it
may

may not be possible, in either of these modes of experimenting, to collect *all* the water into which the two kinds of air may be converted. The nature of this kind of vapour into which water may be changed, and which is not readily condensed by cold, is very little understood, but well deserves the particular attention of philosophers*. Even mercury will evaporate, so as to lose weight, in a degree of heat below that of boiling water.

X That the water collected in the balloon comes from the decomposition of the air, and not from the fresh air circulating through it, was evident from placing balls of hot iron in the place of the flame, and finding that, though the balloon was as much heated by them as by the flame of the burning of the inflammable air, and consequently there must have been the same current of the external air through it, no moisture was found in the balloon.

When, in this manner, I burned inflammable air from pure iron, the water I col-

* Mr. SAUSSURE has made some valuable observations on this subject.

lected

X Much more it must have passed over the flame than that ball was capable of

lected was perfectly free from acid, and the inside of the balloon was quite clear, but when I used *sulphurated iron*, there was a dense white cloud that filled the inside of the balloon. There was also a strong smell of vitriolic acid air, and the water collected was sensibly acid to the taste.

Having found that water is an essential ingredient in the constitution of inflammable air, at least as produced from iron, it still remained to be determined whether, when a calx is revived, and the metal formed, the pure phlogiston only entered the calx, or, together with it, that *water* which was necessary to its form of inflammable air.

In order to ascertain this, I frequently revived dry calces of lead in dry inflammable air, and examined the appearances of moisture afterwards. But notwithstanding all the attention that I gave to the process, I could not be absolutely certain, whether more moisture was left in the vessel, than might have existed *extraneously* in the inflammable air, or whether, when the phlogiston was absorbed, it left behind it any
water

water that had been essential to it, as inflammable air. Appearances were such as sometimes inclined me to think that every thing which constitutes inflammable air goes into a calx, in order to form the metal; so that if this, though a compound thing, be called *phlogiston*, it will still be true that *phlogiston* and inflammable air are the same thing; but, on the whole, I rather think that the water which was essential to the constitution of inflammable air was left behind.

That water, however, may exist in bodies in a *combined state*, without appearing to be water, we know in many cases; but it is in nothing more evident than in the *scales of iron*, than which no substance can have less the appearance of containing water.

But not to give a mere *opinion*, I shall recite the particulars of a few *experiments*, which I made with the view above-mentioned. In 6 $\frac{1}{2}$ ounce measures of inflammable air from iron, I revived lead till it was reduced to 1 $\frac{1}{2}$ ounce measure, care having been taken to make every thing as dry as possible. Some moisture, however, did

did appear, perhaps more than half a grain; but as this air had been confined by water, it was no more than might have been contained in it as an extraneous substance. It ought also to be considered, that it must be exceedingly difficult to expel all moisture by mere heat from such a powdery substance as the yellow calx of lead, without reviving the metal. All chemists well know how firmly moisture adheres to many substances, with which it does not properly unite, and how much heat is necessary to separate them.

Again, in 6½ ounce measures of inflammable air from iron, I revived lead till there remained 0.9 of a measure, and there was hardly any more moisture than I had reason to think might have been in the vessel, independently of what was contained in the inflammable air; and in order to enable myself to judge of this, I melted an equal quantity of the same minium, under a dry glass vessel with common air, when a little moisture appeared on the inside of the glass, about as much, I thought (for I could

could only judge by my eye) as when I had revived the lead from that minium in inflammable air. The quantity of lead revived was only 16 grains, but a good deal of the minium had been made black in the process.

Lastly, I exposed some calx of lead to the heat of the lens in inflammable air, received immediately from the vessel in which it was generated from iron and oil of vitriol, because this contains less water than that which has been received in water and confined by it; and when 6 or 7 ounce measures of the air were absorbed, I could not suppose, from the appearance, that the water could be more than a quarter of a grain. However, when I repeated the experiment once more, I thought there might be about half a grain of water, which is more than I can well account for, without supposing that the water which was necessary to the constitution of inflammable air, and which I suppose to be about half its weight, was left behind when the pure phlogiston revived the calx. This, therefore, is the opinion

opinion to which I am inclined; so that I do not think that any water enters into the constitution of any of the metals.

SECTION V.

Of the Production of Inflammable Air from different Substances.

IT is probable, that every substance which contains *phlogiston* may be made to yield inflammable air. But for this purpose they require different modes of treatment, according to their respective natures. If the substances be *fluid*, heat applied to them directly makes no change in their constitution; but when they are made to pass, in the form of *vapour*, through tubes previously made red-hot, in which they are necessarily exposed to a red heat themselves,

they are readily decomposed; and the quantity of inflammable air that was yielded by some of them, in this mode of treatment, appeared to me rather extraordinary.

I began these experiments with *spirit of wine*, having an apparatus proper to receive any *water*, or other fluid, that might be formed, or condensed, in the process. From two ounce measures of spirit of wine, which was made to pass, in vapour, through a red-hot earthen tube, I got about 1900 ounce measures of air, which was all inflammable, without any mixture of fixed air in it, and which burned with a lambent blue flame. Thirty ounce measures of this air weighed eight grains less than an equal quantity of common air. In this process I collected 0.35 of an ounce measure of water.

In this experiment the air would have weighed

	633 grains
the watery residuum	168
	801

and the spirit of wine would have weighed 821; so that the produce was pretty nearly what

what might have been expected from the materials, the nature of the process considered.

I then proceeded to subject to the same process a quantity of *vitriolic æther*; and making an ounce measure of it pass through the hot earthen tube, almost filled with pieces of broken retorts, or crucibles (in order to make a greater quantity of red-hot surface) I collected one tenth of an ounce measure of water, and 740 ounce measures of air, all inflammable, without any mixture of fixed air. It burned with a large lambent white flame, like that of wood in a common fire, and would not explode with any mixture of dephlogisticated air. Twenty-nine ounce measures of this air weighed five grains less than an equal bulk of common air.

In the next place, I made some vapour of *Spirit of turpentine* pass through the hot earthen tube, and procured from it a quantity of inflammable air, that was very turbid, like black smoke. But the black matter contained in it was soon deposited on the

surface of the water in which it was received. This also contained no fixed air, and burned with a lambent flame, but much less luminous than that in the preceding experiment. The smell of this air was so exceedingly offensive, that, the apparatus being a little deranged, I discontinued the process before I had ascertained the quantity of air, and without collecting any water, which I suppose would have been given. Thirty ounce measures of this air weighed eight grains less than an equal quantity of common air.

I did not repeat this experiment with *olive oil*, being apprehensive that the process would be even more offensive than that with the spirit of turpentine, and nothing material depending upon it. But, upon another occasion, I mixed an ounce of olive oil with 874 grains of *calcined whiting*; and subjecting it to a red heat in an earthen retort, I got from it near 300 ounce measures of air, and should probably have got much more, if there had been more whiting in proportion to the oil. The first portion
of

of this air burned with a large white flame, and the last with a slight lambent blue one, exactly resembling the varieties in the process for extracting air from wood; so that there can be no doubt, but that it is the *oil* in the wood that gives the air. That excellent philosopher, Mr. Volta, was the first who hit upon this method, or a similar one, of getting inflammable air from *oil*; and he has given a large account of its peculiar properties.

From other experiments that I made, it appears, that *water* is essential to the formation of inflammable air. In all the liquid substances mentioned above, the water that enters into their composition is sufficient for the purpose; and spirit of wine, and æther, appear to contain more water than is necessary. But when the substances are *dry*, and water does not enter as a necessary ingredient into their composition, water must be introduced into the process. This is the case with all the *metals*, and it is no less so with *sulphur*, *arsenic*, and probably other substances

substances of a similar nature, which mere heat only sublimes.

Transmitting steam over a quantity of *sulphur*, which was melting in a hot earthen tube, I procured from it a considerable quantity of inflammable air, without any fixed air; and by analysis it appeared to be of the same quality with that which is procured from iron by oil of vitriol. This process is rather troublesome, on account of the sulphur subliming, and filling up the tubes through which the air is conveyed.

I then repeated the same process with *arsenic*, and from this substance also I procured air in great plenty. One-seventh of it was fixed air, but the rest strongly inflammable, and the smell of it could not be distinguished from that of phosphorus. Twenty ounce measures of this air weighed $4\frac{1}{2}$ grains less than an equal quantity of common air. This experiment was no less troublesome than the preceding, on account of the arsenic subliming, and choking up the tubes.

Having

Having found a very heavy kind of inflammable air by heating the *scales of iron* mixed with *charcoal*, I made the following experiment in order to ascertain the quantity of air that might be procured from a given quantity of these materials. Mixing two ounces of the *scales*, or *finery cinder* (which I found to be the same thing) with one ounce of perfect charcoal, I got from it, in an earthen retort, 580 ounce measures of air, one-tenth of the first part of which was fixed air; but afterwards it was all inflammable. The substances were pretty firmly concreted together, and weighed 1044 grains; so that the loss of weight was 396 grains, which must have been very nearly the weight of the air procured. Forty ounce measures of this air, freed from all fixed air, weighed two grains more than an equal quantity of common air.

Besides the *water*, which seems to be essential to the constitution of inflammable air, this species of air readily imbibes more water, which adds greatly to its specific

gravity. This seems at least to be indicated by the following experiment. Filling a dry bladder with inflammable air, received immediately from the vessel containing the iron and diluted oil of vitriol, from which it was generated, I found that 30 ounce measures of it weighed more than seventeen grains less than an equal bulk of common air; but when I weighed that inflammable air of the same kind which had been confined by water in the same bladder, it was only fourteen grains less than an equal quantity of common air. This I repeated several times with the same result. This air, therefore, could only be about three times lighter than common air; whereas the other was more than ten times lighter.

Having frequently examined the specific gravity of inflammable air which has been long confined by water, by weighing it in a bladder, and then pressing out the air, and weighing it when empty (which has the same effect as weighing it full of common air) I have seldom found such air more
than

than five times lighter than an equal portion of common air; so that the specific gravity of this air is soon doubled by being kept in these circumstances.

SECTION VI.

Of sulphurated inflammable Air.

THERE is no kind of air which admits such a variety of modifications as the inflammable; nor shall we think this extraordinary, when we consider that phlogiston, which is the distinguishing ingredient in it, enters into a greater variety of combinations with *solid substances* than perhaps any other principle in nature, and is the cause of a greater variety of properties in them. Spirit of wine, oil, sulphur, charcoal, and metals, are substances as different from each other, both in their external appearance, their degrees of consistence, and
other

other chemical properties, as any things in nature, and yet the principal ingredient in them all is the same phlogiston, as may be proved by the actual transferring of it from any one to any other of them. Inflammable air likewise extracted from each of these substances, as also that from putrid vegetables, and by other processes, of which an account has been given in the preceding section, are all remarkably different, and appear to be so, as we shall presently see, when they are decomposed. I shall now give an account of another species of this kind of air, which I term *sulphurated*, from the strong smell that it has of sulphur, or rather liver of sulphur, and its being loaded with a greater quantity of matter, which, though at the first black, yet on exposure to the air presently assumes a yellowish colour. I shall recite the experiments in which I observed this peculiar species of air, in the order in which I made them, noting the other appearances that accompanied them, though they have not any immediate relation to the *air* of which I am treating.

When

When I was engaged in that course of experiments in which steam, and the vapour of various fluid substances, was brought into contact with solid substances red-hot, I treated *manganese* in this manner, and especially a quantity with which Mr. WOLFE had formerly furnished me, which was not in powder, but in a large mass, just as it is dug out of the earth. A few ounces of this I put into an earthen tube, open at both ends. But closing one of them with a cork, while the middle part of the tube was red-hot, and the other orifice was furnished with an apparatus proper for collecting the air that might be expelled from it, I received 40 ounce measures of air, of which one-sixth was fixed air, and the rest of the standard of 1.7, lambently inflammable. No more air coming in this disposition of the apparatus, I opened the other end of the tube, and with a proper contrivance for the purpose, sent through it a quantity of steam; in which circumstance air was produced more copiously than before. Of this I received about 50 ounce measures, observing that one-seventh

venth of it was fixed air, and the rest of the standard of 1.8, not lambently, but explosively inflammable. The last portions of this air were very turbid, and the smell of the air, and especially that of the last portion, was very sulphureous, and, I observed, tinged the water of a very dark colour, by depositing in it a quantity of blackish matter. However, the air itself became presently transparent, and had no other appearance than that of any other kind of air, when I left in my trough a jar filled with it.

Having been intent on some other experiments, I was surprised to find, on looking on the jar about ten minutes afterwards, that it was quite black, so that I could see nothing in the inside of it. In order to observe how it came to be so, I afterwards filled another jar with this kind of air, and observed that when the water was well subsided, black specks began to appear in different places, and, extending themselves in all directions, at length joined each other, till the whole jar was perfectly black, and the glass quite opaque. When
this

this was done, I transferred the air to another clean jar, and it soon produced the same effect upon this, though it never became so black as the jar in which it had been first received. It also frequently happened that only the lower part of the jar would become black, as if this matter, with which it was loaded, had kept subsiding, though invisibly, in the mass of air, and occupied the lower regions of it only, leaving the upper part entirely free from it. When the vessels thus tinged black were exposed to the open air, that colour presently disappeared, and a yellow or brown incrustation was left upon it.

Thinking, from this circumstance, that this black coating consisted of some volatile phlogistic matter, I placed the jars which had this black tinge with their mouths inverted in vessels of water, in order to observe the effect which the change of colour might have on the common air contained in them. In these circumstances the black tinge presently went off, and was succeeded by the yellow colour, but without producing any sensible change in the air. In some cases, however,

however, I thought that it was injured; but it was by no means so much so as I had expected. After depositing this black matter, the air still retained its sulphureous smell, and as far as I can judge, will never entirely leave it.

It is by no means the universal property of manganese to yield this sulphurated inflammable air, but must have been owing to something peculiar to this specimen, and perhaps to something accidentally mixed with it. For when I repeated the experiment with other manganese, which I had from a glass-house, in which it is used, I had no such appearance. From four ounces of this manganese, treated as the preceding, I got without steam, 256 ounce measures of air, of which about one-tenth was fixed air. Then sending steam over it, I got more air, but in no great quantity, about ten ounce measures in an hour; though probably much more might have been procured, if the process had been continued. This was dephlogisticated; for, mixed with two equal measures of nitrous air, the standard was

0.28,

0.28, which shews that it was exceedingly pure. But one-tenth of this was fixed air, as in the former portion, which agrees with the experiments I formerly made with this substance when I found that heat alone would expel from it a quantity of very pure air.

The next time that I got this sulphurated inflammable air, was as unexpected as the preceding; and this experiment was the first thing that gave me any insight into the nature of it. Having occasion to make a large quantity of inflammable air, instead of fresh burnings of iron, I happened to take some, parts of which had been heated by a burning lens in vitriolic acid air, in which, as I have observed, it melts with great readiness, and gathers into balls. When this iron was dissolved in diluted oil of vitriol, though there were only a few pieces in the quantity that I used which had been melted in this manner, the water in which the air was received was very black, and deposited more sediment than in the experiment with the manganese. The jars also which contained

it

it were presently black as ink, but became yellow when exposed to the open air. This inflammable air had also the same offensive sulphureous smell; so that there could be no doubt of its being the same kind of air which I had got from Mr. Woulfe's manganese. There was in it, however, a mixture of vitriolic acid air, as I perceived when I burned a large quantity of it in a glass balloon, in order to collect the water that might be produced in this process. All the inside of the balloon was filled with a dense white cloud, all the time that the air was burning in it, and the water produced was very sensibly acid. In reality, the same effects were produced as if sulphur had been burned in the vessel.

As I had no doubt, but that the iron which had been melted in vitriolic acid air was the same as what is called *sulphurated iron*, or iron with which sulphur is incorporated, I now completely ascertained it by making a quantity of sulphurated iron, dipping it when red-hot into melted sulphur. This iron, treated as the other had been, yielded

yielded exactly such air as I have been describing, so that I could have no doubt with respect to the real origin of it.

When I decomposed this air, by firing it with an equal quantity of dephlogisticated air, the diminution of bulk was the same as when I used the common inflammable air, so that it did not appear to contain either more or less phlogiston; but there was a small quantity of *fixed air* produced, which is never the case with inflammable air procured with oil of vitriol, though it is when it is procured from iron with spirit of salt.

When the sulphurated inflammable air is received in vessels containing mercury, there is very little black matter deposited from it, but it appears when it is transferred into vessels containing water.

Though jars thinly coated with this black matter become yellow when exposed to the open air, this is not the case with that which is collected from the water in which the air has been confined. For when the water is evaporated from it, it adheres to

the evaporating vessel in the form of a perfectly black incrustation. This substance, though it does not burn blue on a hot iron, yet shews evident signs of containing sulphur. For when the nitrous acid has taken from it its superfluous phlogiston, it has both the colour and the smell of sulphur.

SECTION VII.

The Analysis of different Kinds of inflammable Air.

BEFORE I proceed to the analysis of the different kinds of inflammable air, which is the subject of this section, I shall observe, that the purest we can procure (which is that from metals by solution in the mineral acids, or rather that by means of steam from red-hot iron) seems to consist of phlogiston and water, and that neither

ther *acid* nor *alkali* is a necessary ingredient in it; though, when it is produced by means of either of them, a small portion of either may be retained in it, as an extraneous substance. That this, however, is the case, has been very clearly shewn by Mr. SENEBIER, though I think that the production of inflammable air by means of iron and steam only, without either acid or alkali, sufficiently proves that his hypothesis of inflammable air necessarily requiring some saline basis cannot be well founded.

It was, indeed, my own first opinion, that inflammable air consists of acid and phlogiston. Afterwards I adopted the opinion of Mr. KIRWAN, viz, that it is pure phlogiston in the form of air, but at present I am fully satisfied with the opinion of Mr. CAVENDISH, that *water* is an essential ingredient in the constitution of this kind of air. It may, indeed, appear extraordinary that since, according to the hypothesis which at present seems to be well established, water itself consists of phlogiston and dephlogisticated air, this water

should exist in inflammable air as an essential part of it, without being resolved into its constituent principles. But we are as yet ignorant of the principles of combination and cohesion in the parts which compose natural substances, much more than we are with respect to the constituent parts themselves; though it is very possible that when we shall have discovered the latter (in which great progress is making every day) the knowledge of this will be a means of throwing some light upon the former; so that we may be able to tell why any particular substance is solid or fluid, why it exists in the form of elastic air, why metals are of a firmer texture than stones or sulphur, or why some of the metals should be so much harder than others, concerning which the most knowing among us cannot at present pretend to form even a plausible conjecture. All that we can say is, that no mere *difficulty of conceiving* how a thing can be, should be admitted as an argument against any conclusion fairly deduced from proper premises.

That

That no acid is necessarily contained, or at least in any sensible quantity, either in inflammable air, though produced by means of acids, or in the dephlogisticated air of the atmosphere, is evident from the following experiment, which I made with the greatest care. Taking a bason which contained a small quantity of water tinged blue with the juice of tumsole, I placed in it a bent tube of glass, which came from a vessel containing iron and diluted oil of vitriol; and lighting the current of inflammable air, as it issued from this tube, so that it burned exactly like a candle, I placed over it an inverted glass jar, so that the mouth of it was plunged in the liquor. Under this jar the inflammable air burned as long as it could, and when it was extinguished, for want of more pure air, I suffered the liquor to rise as high as it could within the jar, that it might imbibe whatever should be deposited from the decomposition of either of the two kinds of air. I then took off the jar, changed the air in it, and lighting the stream of inflammable

air, replaced the jar as before. This I did till I had decomposed a very great quantity of the two kinds of air, without perceiving the least change in the colour of the liquor, which must have been the case if any acid had entered as a necessary constituent part into either of the two kinds of air. I also found no acid whatever in the water which was procured by keeping a stream of inflammable air constantly burning in a large glass balloon, through which the air could circulate, so that the flame did not go out. Neither was there any acid produced in the decomposition of inflammable and dephlogisticated air in a strong close glass vessel.

With respect to inflammable air itself, I have before observed, that when sufficient care is taken to free it from any acid vapour that may be accidentally contained in it, it is not in the smallest degree affected by a mixture of alkaline air. On the whole, therefore, I have at present no doubt but that pure inflammable air, though it certainly contains *water*, does not necessarily contain any acid. Yet an acid vapour may be easily
diffused

diffused through it, and may perhaps in many cases be obstinately retained by it, as no kind of air seems to be capable of so great a variety of impregnations as inflammable air is.

That there are different kinds of inflammable air, has been observed by most persons who have made any experiments on air. That which has been most commonly observed is, that some of them burn with what may be called a *lambent flame*, sometimes blue, sometimes yellow, and sometimes white, like the flame from wood or coals in a common fire; whereas another kind always burns with an *explosion*, making more or less of a report, when a lighted candle is dipped into a jar filled with it. Of the latter kind is that which is extracted from metals by means of acids, &c. and of the former kind is that which is expelled from wood, coal, and other substances by heat. It has also been observed, that these kinds of inflammable air have different specific gravities, the purest kind, or that which is extracted from iron, &c. being about ten

times lighter than common air, but some of the other kinds not more than twice as light.

The cause of this difference I once thought I had discovered to be the heavier kinds of inflammable air, containing a proportion of *fixed* air, so intimately combined with them, so as not to be discoverable by lime water, while the lightest kind contained no fixed air at all. This hypothesis I formed from decomposing them with common or dephlogisticated air, by the electric explosion. For, after the experiment with the heavier kinds of inflammable air, I always found a quantity of fixed air in the residuum, but none at all after the experiment with the lightest kind.

In order to decompose any kind of inflammable air, I generally mix it with an equal quantity of dephlogisticated air, and then confine them in a strong glass vessel, previously filled either with water or mercury, and I make an electric spark in some part of the mixture, by means of wires inserted through the sides of the vessel, and

remains

M

nearly

nearly meeting within it. By this process, I imagined, that I was able to ascertain two things relating to the constitution of different kinds of inflammable air, viz. the quantity of *combined fixed air* (as I then thought it to be) and likewise the relative quantity of phlogiston contained in each of them. The former appeared by washing the air with lime water after the explosion, and observing how much of them was absorbed, and the latter by examining the residuum with the test of nitrous air, and observing the purity of it. In most of these experiments I made use of dephlogisticated air, in preference to common air, because I could not make some kinds of inflammable air to explode at all with common air. Otherwise I should have preferred this, as being the most nearly of the same quality. However, I always noted the degree of purity of the dephlogisticated air that I made use of before I began any of these analyses.

Finding, however, that in some cases more fixed air was found after the explosion of the two kinds of air, than could possibly have

have been contained in the inflammable air, on account of the weight of it, I was satisfied that there must have been a real *generation* of it, by an union of the inflammable and dephlogisticated air. It is remarkable, however, that some kinds of inflammable air should so readily unite with dephlogisticated air, so as to make a considerable quantity of fixed air; and that others, treated in the same manner, should not do this at all; and also that those which do it should be the heavier kinds of inflammable air. This is a new and curious subject of investigation.

The purest and lightest inflammable air is that which is extracted from iron, and other metals, by a solution in the acids, or by means of steam. One measure of this kind of air, and one of dephlogisticated (such as that, when mixed with two equal quantities of nitrous air, there remained 0.72 of a measure) exploded together in the manner described above, were reduced to 0.6 of a measure, no fixed air was found in the residuum, and when examined with an equal

equal quantity of nitrous air, was reduced to 0.87 of a measure.

With the same dephlogisticated air I examined inflammable air that had been got from a mixture of finery cinder and charcoal, and found, that after the explosion, the two measures were reduced only to 1.85, but that by washing in the lime water, they were reduced to 1.2. Consequently 0.65 of a measure of fixed air had been generated in the process. When this was separated from it, and the remainder examined by nitrous air, it appeared to be of the standard of 0.9; so that the dephlogisticated air had been more injured by this than by an equal quantity of the common inflammable air, though the difference in this respect was not considerable.

In another process with this kind of inflammable air, the diminution after the explosion was to 1.55, and that after the washing in lime water to 0.65 of a measure; so that there had been a generation of 0.9 of a measure of fixed air. In another experiment the first diminution was

was to 1.6, and the second 0.66, so that 0.94 of a measure of fixed air had been produced. And lastly, in another process, the first diminution was to 1.6, and the second to 0.6 of a measure; so that there was a generation of one complete measure of fixed air, and this was a clear proof that it could not have been contained in a combined state, as I at first imagined, in the inflammable air; since then it must have been much heavier than I had ever found it to be; for though I found the specific gravity of it to be something different at different times (and the preceding experiments were made with the air of different processes) I had never found that 40 ounces measure of this air was more than 25 grains heavier than an equal bulk of common air.

This, indeed, is a remarkable circumstance with respect to a species of inflammable air, as it does not appear by the test of lime water to contain any fixed air; but it ought to have weighed more than one-half heavier than common air, to have actually contained in combination all the fixed air

that

that I found after its explosion with the dephlogisticated air. Indeed, if any quantity of inflammable air, of about the same specific gravity with common air (which is the case with that species of it which I am now considering) yield so much as seven-tenths of its bulk of fixed air, in consequence of its explosion with dephlogisticated air, it is a proof that at least part of that fixed air was generated in the process, because seven-tenths of such fixed air would weigh more than the whole measure of the inflammable air.

Inflammable air from *spirit of wine* (made by transmitting it in vapour through a red hot earthen tube) being analysed in the manner above mentioned, one measure of it, and one of the same dephlogisticated air that was used in the former experiment, were reduced to one measure, and by washing in lime water to 0.6 of a measure; so that four-tenths of its bulk of fixed air had been generated in the process. The standard of the residuum was 1.7; so that the dephlogisticated air had been injured much more than in either of the former processes, and consequently

quently it must have contained more phlogiston.

I found considerable variations in the experiments with this, as well as with some other kinds of inflammable air. For, in another process, in which the earthen tube had been filled with bits of crucibles (in order to expose more red surface to the vapour of the spirit of wine) the first diminution was to 1.6, the second to 1.4, and the standard of the residuum was 1.84. In another process with this kind of air, the first diminution was to 1.2, and the second to 0.9.

Having procured a quantity of inflammable air, by transmitting steam over red hot *platina*, I analysed it in the same manner, and found that the two measures were reduced by the explosion to 0.72. It contained no fixed air, and the residuum was of the standard of 0.9.

Inflammable air, procured by making steam pass over melted brimstone, being examined in the same manner, the first diminution was to 0.6, and no fixed air was found

found in it. In this respect it seems to have been the same thing with inflammable air from iron, but the standard of the residuum was 0.95; so that it seems to have contained more phlogiston. But I have found the standard of this residuum subject to considerable variations, such as I could not well account for.

Inflammable air procured in the same manner from melted *arsenic*, appeared to be very different from that which was extracted from brimstone. For the two measures were reduced by the explosion to 1.15, and by washing with lime water, to 0.95; so that one fifth of its bulk of fixed air had been generated. The standard of the residuum was 0.82.

At the same time I examined some inflammable air, that had been made by heating bits of crucibles in *alkaline air*, and found that the two measures were reduced by the explosion to 0.96 of a measure, that the residuum contained no fixed air, and was of the standard of 0.8.

The

The inflammable air that is made from *æther*, by transmitting the vapour of it through a red hot earthen tube, very much resembles that which is got from spirit of wine. The two measures were reduced by the explosion to 1.36, and by washing in water to 1.2; so that 0.16 of a measure of fixed air had been generated, and the residuum was of the standard of 1.9.

Inflammable air procured by transmitting steam over red hot *charcoal of metals*, in the same manner as it is got from other charcoal, produces a considerable quantity of fixed air. For when the experiment was made with this air, the first diminution was to 1.12, and the second to 0.8; so that 0.32 of a measure of fixed air was generated, and the standard of the residuum was 1.9. This analysis was of the first portion that came in the process. The second was something different. For with this, the first diminution was to 1.0, and the second to 0.75, the residuum being the same as before, viz. 1.9. Thirty ounce measures of this air weighed

weighed 8 grains less than an equal bulk of common air.

Analyzing the inflammable air from *coak*, or the charcoal of pitcoal produced by steam, the first diminution was to 1.15, and the second to 0.95; so that one fifth of its bulk of fixed air was generated. The standard of the residuum was 1.9. But I must observe, that the dephlogisticated air with which this experiment was made, was so impure as hardly to deserve the name. For two measures of nitrous air and one of this, occupied the space of two measures. But this circumstance may not affect the quantity of fixed air generated in the process. Thirty ounce measures of this air weighed ten grains less than an equal bulk of common air.

Analyzing the inflammable air that was produced in the same manner from *spirit of turpentine*, the first diminution was to 1.7, and the second to 1.6; so that only one tenth of fixed air was produced. The residuum was of the standard of 1.9. Thirty ounce measures of this air weighed eight

grains less than an equal bulk of common air.

When the vapour of spirit of wine was made to pass over melted metals, inflammable air was produced from it just as if no metals had been concerned; but when I examined the air that was procured in this manner, it did not appear to be quite the same with that which came from pure spirit of wine. Analyzing the air that was produced in a process with *copper*, the first diminution was to 1.7, the second to 1.56, and the standard of the residuum was 1.78. Thirty ounce measures of this air weighed seven grains less than an equal bulk of common air.

In another process with inflammable air, procured in this manner, the first diminution was to 1.55, the second to 1.48, and the residuum was of the standard of 1.86. Thirty ounce measures of this air weighed $8\frac{1}{2}$ grains less than an equal bulk of common air. This air, it is observable, produced much less fixed air than the other, and it was also specifically lighter than it.

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When this process was made with the air procured by transmitting vapour of spirit of wine over melted *silver*, the first diminution was to 1.9, the second to 1.78, and the standard of the residuum was 1.9. Thirty ounce measures of this air weighed eight grains less than an equal bulk of common air.

In the analysis of the air procured by this process from *lead*, the first diminution was to 1.78, the second to 1.6, and the standard of the residuum was 1.78.

I examined, at the same time, inflammable air procured from *bones*, and also from *charcoal*, viz. by transmitting steam over them when they were red hot in earthen tubes, when all air had been previously expelled from them by heat. With the former, the first diminution was to 0.67, and the second to 0.58; so that the fixed air produced was extremely inconsiderable, viz. only 0.09 of an ounce measure. The standard of the residuum was 1.47. In the experiment with the air from *charcoal*, the first diminution was to 1.5, and the second

to 0.74 ; so that the fixed air was 0.76, and the standard of the residuum was 1.7. From this experiment it may be inferred, as mentioned before, that inflammable air from *bones*, is a kind of medium between that from *metals*, and that from *charcoal*. In another process with air from charcoal, the first diminution was to 0.82, and the second to 0.63, and the standard of the residuum was 1.37.

I made the following experiment to ascertain how much phlogiston is contained in inflammable air from charcoal. In five ounce measures of this kind of air, I revived lead from massicot till it was reduced to three fourths of an ounce measure, when the lead revived weighed ten grains, and there remained one ounce measure of fixed air. But the minium itself yielded a little fixed air.

It is observable, that when wood is heated in an earthen retort, the first air that comes over is considerably different from that which comes in the middle, or at the end of the process. Indeed the properties of it

it are continually changing during the whole process. The first portion burns with a lambent white flame, like that from burning wood in an open fire; afterwards the flame is blue, and towards the end of the process it is considerably explosive, almost like air from iron. Also the air that comes over first is very turbid, owing perhaps, to oily, and other matters, that are rendered volatile by heat.

Having procured air from dry beech-wood, I examined, in the method described above, the first portion of it, and also one of the middle ones. The former I found to contain $4\frac{1}{2}$ tenths of its bulk of uncombined fixed air, the second portion only two tenths. Afterwards it is well known that air procured in this manner ceases to have any uncombined fixed air in it.

When I examined the first portion of air, after the uncombined fixed air had been separated from it, the first diminution was to 1.36, and the second to 0.9; so that 0.46 of a measure of fixed air was generated, and the standard of the residuum was 1.9.

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When the second portion was examined, after the uncombined fixed air was likewise separated from it, the first diminution was to 1.66, and the second to 1.46; so that the fixed air generated in the process, was 0.2 of a measure, that is, less than in the former experiment, and in nearly the same proportion as the uncombined fixed air had been. The standard of the residuum in this last case was 1.15. At the same time repeating the experiment with the same dephlogisticated air and inflammable air from iron, the diminution after the explosion was to 0.55, and the standard of the residuum was 1.48, which is the usual result of the decomposition of inflammable and dephlogisticated air, when both of them are as pure as they are generally procured.

There was a great quantity of fixed air produced by the decomposition of some inflammable air extracted from some rich *mold* in a gun-barrel, which Mr. YOUNG was so obliging as to send me. It burned with a lambent blue flame, and had a peculiarly

cularly offensive smell, the same, as he observes to me, that is yielded by air procured from putrid vegetables. Of this air one twentieth part is uncombined fixed air. When this was separated from it, and the remainder decomposed with dephlogisticated air, the first diminution was to 1.4, and the second to 0.67; so that there was a generation of 0.73 of a measure of fixed air. The residuum was of the standard of 0.6.

The inflammable air that is procured from *cast iron* has a peculiarly offensive smell. On this account I had imagined that it might contain more phlogiston than common inflammable air, so as to absorb more dephlogisticated air than the other. But this did not appear to be the fact. For when I mixed one measure of each of the kinds of inflammable air with four measures of common air, the diminution after the explosion, was the very same with both, viz. to 1.56.

Though I think it to be unquestionable, from the preceding experiments, that part at least of the fixed air which is found

on the decomposition of lambent inflammable air is *generated* in the process ; yet, in another experiment that I made, it should seem that fixed air, or the elements, as we may say, of fixed air, may enter into the composition of inflammable air, and actually remain there, without being discoverable by lime-water.

I took a quantity of *slaked lime*, which had been long kept close corked in a bottle, and found that when it was heated in an earthen retort, it gave air, of which one fifth was for the most part fixed air ; but in the gun-barrel the same lime yielded no fixed air at all, but a great quantity of pure inflammable air, of the explosive kind, like that which is got from iron alone with water. That the water in slaked lime will enable the iron of the gun-barrel to yield inflammable air cannot be questioned, but then what became of the fixed air which the same lime would have yielded in an earthen retort?

This experiment appearing rather extraordinary, I repeated it, with all the attention

tion I could give to it, and had the following result. I heated three ounces of flaked lime (but which had been some time exposed to the open air) in an earthen tube, and got from it 14 ounce measures of air, of which only $2\frac{1}{2}$ measures remained unabsorbed by water, all the rest being fixed air. This residuum was slightly inflammable, but not perfectly phlogisticated. For, examining it with the test of nitrous air, the standard of it was 1.6.

Immediately after this I heated another three ounces of the same flaked lime in a gun barrel, and got from it about 20 ounce measures of air, of which no part was fixed air, but all inflammable. I expected, however, to have found fixed air on the decomposition of this inflammable air with dephlogisticated air; but after this process it appeared to be exactly such inflammable air as is procured from metals by the mineral acids, or rather by steam. For the diminution of the two kinds of air was the same, and though there was some appearance of fixed air in the residuum, it was not so much

much as is found after the decomposition of the inflammable air that is procured by means of spirit of salt. In this case, therefore, there are no less than $11\frac{1}{2}$ ounce measures of fixed air absolutely unaccounted for, unless it be supposed that it was resolved into its constituent principles, phlogiston or dephlogisticated air, and that the latter was decomposed as it was produced. This, therefore, I think must have been the case.

Thinking that the two kinds of air might incorporate when one of them was generated within the other, I filled a gun barrel previously full of mercury with fixed air, and put the closed part of it into a hot fire. Inflammable air was accordingly produced, but when the fixed air was separated from it, it exploded just like inflammable air from iron only.

I made an experiment something similar to this, by heating iron turnings in five ounce measures of fixed air, when the quantity of it was increased about one ounce measure, and there remained 1 and $\frac{3}{4}$ of an ounce measure unabsobered by water. This was inflammable,

flammable, and burned with a lambent blue flame, not like inflammable air from iron. It should seem, therefore, that in this experiment, $\frac{3}{4}$ of an ounce measure of inflammable air had been formed by the union of the fixed air with the phlogiston from the iron. This experiment I repeated with the same result, and I farther observed, that though the inflammable air procured in this manner did not appear, by the test of lime water, to contain any fixed air, yet when it was decomposed, by being fired together with an equal quantity of dephlogisticated air, fixed air was found in the residuum. For the first diminution was to 1.45, and one-third of this residuum was fixed air. From this fact it should seem that, though in some cases, fixed air must be *generated* by the decomposition of inflammable and dephlogisticated air, yet that inflammable air, when thus produced in contact with fixed air, may *combine* with it, so as to be properly *contained* in it, and in such a manner, as that it cannot be discovered by lime water.

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I also observed, after Mr. METHERIE, that though no fixed air be found on the decomposition of dephlogisticated air with inflammable air procured by means of oil of vitriol, a small quantity is produced when the inflammable air is procured by means of spirit of salt. I did not find, however, more than a fortieth part of the residuum to be fixed air, when I decomposed equal quantities of the two kinds of air.

I found some appearance of fixed air, but even less than in the last-mentioned experiment, when I heated red precipitate in inflammable air procured by means of spirit of salt. For there was a slight turbid appearance in the lime water to which it was admitted, but I could not measure any certain diminution of bulk from agitation, as in the other case.

SECTION VIII.

The Analysis of Alkaline Air.

THAT alkaline air contains phlogiston appears both by the calces of metals being revived in it, and by its being converted into a species of inflammable air, by the electric explosion, and other processes. By the following experiments it seems to contain less phlogiston than an equal bulk of inflammable air from iron, and about one-fourth of its bulk of phlogisticated air. But it is not possible to ascertain this with perfect exactness, from the nature of the processes. I shall therefore recite the experiments in the order in which they were made, some of them being more, and others less accurate.

I first observed that some *massicot*, which I used in some of these experiments, contained a portion of fixed air, which could be expelled from it by heat; but when I

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melted it in alkaline air, by which means the lead was always revived, I found no fixed air in the residuum, but only pure phlogisticated air. This, however, is easily accounted for, as the alkaline air would immediately unite with any fixed air that should be set loose in the process.

With respect to the quantity of lead revived in alkaline air, it is difficult to be ascertained, on two accounts. In the first place, some of the calx is blackened, and imperfectly revived; and again, if particular care be not taken, the lead that is revived will be dissolved in the mercury, by which the air must necessarily be confined. To prevent this last inconvenience, I put the powdered massicot into small earthen cups, which I contrived to place with their mouths upwards, so that when the lead was revived, by means of a burning lens, it would remain in the cup, and not mix with the mercury by which it was supported. On these accounts, however, the largest quantity of lead that was revived must always be considered as nearest the truth.

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In three ounce measures of alkaline air, I first revived six grains of lead, then in $3\frac{1}{2}$ 16 grains, in $2\frac{1}{2}$ 13 grains, and in $3\frac{3}{4}$ 12 grains. In five ounce measures I revived 10 grains, besides blackening a quantity of it, and there remained one ounce measure of phlogisticated air. But the experiment which I made with the most care, and on which I place the greatest dependence, was the following.

In $7\frac{1}{2}$ ounce measures of alkaline air, I revived $26\frac{1}{2}$ grains of lead, and there remained $2\frac{1}{2}$ ounce measures of phlogisticated air. In this proportion 100 ounce measures of alkaline air would revive 352 grains of lead; whereas 100 ounce measures of inflammable air from iron would revive 480 grains of lead. But alkaline air is resolvable into considerably more than twice its bulk of inflammable air, and therefore it might have been expected that it would have revived much more lead. Would it not, therefore follow from this, that phlogisticated air contains a very great quantity of phlogiston, many times more than an equal

equal bulk of pure inflammable air. Supposing alkaline air to be resolvable into no more than twice its bulk of inflammable air, a hundred ounce measures of it ought to revive 960 grains of lead. But in the proportion mentioned above, it would revive no more than 608 grains. As much phlogiston, therefore, as would revive 342 grains of lead (which is the difference between these two numbers) should be contained in about 25 ounce measures of phlogisticated air. Consequently 100 ounce measures of phlogisticated air would revive 1368 grains of lead, which is more than twice as much as an equal quantity of pure inflammable air would revive. It must, however, be considered, that the inflammable air, into which alkaline air is resolvable, is not the same with that which is procured from iron, and may contain much less phlogiston.

To these experiments, on the revival of lead in alkaline air, I shall subjoin an account of some that were made with other calces, heating them all, when confined by this air, by means of a burning lens.

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Glass of antimony absorbed a part of the alkaline air, and there was the appearance of metal in several small globules, whereas I could get no such appearance from the same substance in inflammable air.

By heating scales of iron in alkaline air, it was much increased in quantity, and then I found it to contain no fixed air, but the whole was strongly inflammable. No water was produced in this process. The iron was revived, being perfectly soluble in diluted oil of vitriol; and from being 24 grains had lost one grain in weight.

As alkaline air evidently contains phlogiston, and as such was capable of reviving lead, and no doubt the other metals, I had the curiosity to make use of it in other circumstances in which I had before used inflammable air; and among other things I tried the effect of heating red precipitate in it. That the mercury should be revived in this process is nothing extraordinary, because it will be revived from the same substance in any kind of air, and even in *vacuo*, the heat applied to it expelling from it the

dephlogisticated air which it contains. But there is one circumstance attending this experiment which I cannot satisfactorily account for. At the same time that the metal revives, a considerable quantity of water is produced, whereas little or none is found when the same substance is revived in inflammable air. It has even run down in drops in the inside of a vessel which contained about five ounces measures of the air. The air which remained after this process was about half as much as the original quantity of alkaline air, and it had in it a quantity of undecomposed dephlogisticated air. For with two equal measures of nitrous air, the standard of it was 1.4, and there was nothing inflammable in it.

Again, I threw the focus of the lens upon red precipitate, in alkaline air, till three measures of it were reduced to two. Water was produced in the process, and the air that remained was considerably dephlogisticated; the standard of it, with a mixture of two equal measures of nitrous air, being 1.7.

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But in another experiment of this kind, there must have been a quantity of inflammable air set loose from the alkaline air, as well as deplogified air from the red precipitate; because, after the operation had continued some time, there was a violent explosion within the vessel; which threw it many feet perpendicularly into the open air, as I was holding it in my hands. In this experiment I had been particularly careful to make every thing concerned in it as dry as I possibly could, in order to satisfy myself with respect to the production of the water which I had found before; and there was time enough before the explosion to observe that water was certainly produced in the process. To appearance the quantity of air was never much diminished.

That water should be produced when the scales of iron are heated in alkaline air, which I found to be the case, is not extraordinary, as the same is found on heating this substance in inflammable air; and it appears to contain within itself the water that is expelled from it. Also though, in

the experiment with red precipitate and alkaline air, neither of the substances contain the water; yet each of the *elements* of water are contained in them, viz. the dephlogistified air in the red precipitate, and the phlogiston in the alkaline air. But the difficulty is how to account for the formation of water in this case, and the non-formation of it when inflammable air is used. The difference of circumstances in the two cases is, that the inflammable air is ready formed in the one case, whereas the elements of it only are contained in the alkaline air. But how this difference in the circumstances should occasion this difference in the result I cannot tell; so that this, together with many other things, must remain to be investigated by future experiments.

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SECTION IX.

Of the Conversion of Alkaline Air into Inflammable Air.

I FORMERLY found that, by taking the electric spark in alkaline air, a very remarkable change was made in it, as from being a species of air most readily absorbed by water, it became immiscible in water, being proper inflammable air, and nearly three times its former bulk. I had then only observed the simple *fact*, but had made no progress in the investigation of the *cause*. I have since made a few steps towards it, and have found that the change is produced by *heat*. But in what manner it operates to produce this effect remains to be enquired into.

When I reflected upon this change of the alkaline air by the electric explosion, I considered that it might be produced either by *electricity as such*, or by the *light*, or the

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heat, which accompany the experiment. In order to try the effect of *light*, I exposed a small quantity of alkaline air to a strong light made by a burning lens, for several hours. But this made no sensible change in the quantity of it. I therefore, concluded that the proper agent in this process was not light.

I did not immediately proceed to try the effect of heat, but observed it by accident some time afterwards, in the following manner. At the time that I was reviving metals in inflammable and alkaline air, I put a quantity of ochre into the latter, expecting to revive it in the form of iron; and in this I seemed to succeed, as the ochre became black in the course of the process. But what struck me the most was, that instead of perceiving the quantity of air to decrease, as I had expected, there was very soon a visible increase of it. Examining the air, I found it to contain no fixed air, and to be all strongly inflammable.

Still attending to other circumstances besides the change of the alkaline air, I proceeded

ceeded to heat in it scales of iron, and iron itself, of different kinds, and still found that the air was increased in quantity, and changed in its quality. But in this way I could never increase the bulk of the air more than twice, and seldom quite so much. Also, after the process, when I could make no farther change in the air, I still perceived a very strong smell of volatile alkali. The iron had undergone no change that I could discover, so that I concluded that the air had been affected by means of *heat* only.

To ascertain this, I then proceeded to heat in the alkaline air bits of dry crucibles, or of earthen retorts, which had been just before exposed to very great heats, so that they could not be supposed to give out any air in this process, and therefore could only serve to produce a strong heat in the alkaline air. The result seemed to confirm my supposition. For in these experiments with earthy substances the effect was always the same as when I had made use of ochre or iron. The air was increased in

bulk, but the quantity not quite doubled, and it was become inflammable.

The bits of *white* earthen ware having always become *black* in this process, I suspected that this kind of substance might have affected the air in some other way than by merely communicating heat to it. To try this, I used the same small bit of crucible again and again, and finding the same effect from heating it at the last, as at the first, I could not help concluding that the change produced in the air had been effected by means of *heat* only.

In all these experiments, however, there was a strong *light* as well as an intense heat concerned. In order to ascertain, therefore, whether the light, at least in conjunction with heat, might not have had some effect in this change of the air, I heated a quantity of it in a coated green glass retort. The mouth of the retort was plunged in a basin of mercury, and I received in a glass tube filled with water all the air that was driven out of the retort by heat. At first it was all absorbed by the
water,

water, being merely alkaline air expelled by rarefaction. But when I concluded that the bulb of the retort was become red-hot, the bubbles that were driven out were not wholly absorbed by water, and by degrees no part of them was absorbed. This air I examined, and found it to be inflammable. The retort was melted in the process, so that I could not examine the air that remained in it; but my purpose was sufficiently answered, as I saw no room to doubt, but that the change of the alkaline into inflammable air was produced by *mere heat*, without the aid of light, if a red heat can be so termed. How the heat operates in this case I cannot tell.

It was observable, that whenever I changed alkaline air into inflammable, by heating in it bits of crucibles or retorts, into the composition of which *clay* enters, they always became black. There was room, therefore, to suppose that it was not by means of *mere heat* that this change was made, but that something might be deposited from the air, which might attach itself

self to the clay. Indeed, if this was not the case, I do not see why the clay should become black; though, perhaps, part of the same phlogiston which forms the inflammable air may be attracted by the red-hot clay, without there being any proper decomposition of the air.

That this is the case, seems probable from an experiment in which I used white *porcelain*, instead of bits of earthen ware, into the composition of which clay enters. For this substance did not become black in the process, and yet inflammable air was produced. But I must observe, that a considerable *time* was requisite to produce this effect in these circumstances, and that it was not much inflammable air that I was able to procure in this way. It was, however, in winter, and the heat that I could apply was not great.

SECTION X.

Of the Connexion between Alkaline Air, Volatile Alkali, and Inflammable Air.

IN some of my former publications I observed, that iron which had long rusted in nitrous air, gave out a strong smell of volatile alkali. I have repeatedly observed the same since that time, and also some other curious particulars relating to the experiment.

In all the cases in which I had before observed this smell of volatile alkali, the iron had been exposed to the nitrous air a very long time. But this effect is produced much sooner by putting the iron into a weak solution of copper in the nitrous acid, such as remains after making nitrous air, and such as I now commonly use in order to procure dephlogisticated nitrous air. A phial containing some of this
iron,

iron, which had been used not more than once for this last mentioned purpose, having been kept close corked about two months, was accidentally broken; when some pieces of the iron were found covered with a green rust, and these had a strong smell of volatile alkali.

Time, however, is requisite to produce this effect. I put some of this iron (which was of the turnings) moist, from a solution of copper in the nitrous acid, into a phial; and stopping it close, left it to stand with its mouth immersed in water. At that time it had no smell, nor had it any a month afterwards; but examining it again after two months, the smell of volatile alkali was pretty strong.

Having, in an early period of my experiments, been much struck with the change of nitrous air, in consequence of the contact of iron, I kept several large bottles filled with small pieces of iron, which I occasionally supplied with nitrous air. The bottles should be strong ones, because the iron in rusting swells, and is apt to break the

the phials, though I do not know that this ever happened except when the iron was quite dry. The consequence of exposing nitrous air in this manner had always been, in the first place, a diminution of something more than one-third of the quantity of it, in which state a candle would burn in it with an enlarged flame; and when well washed in water it would be farther diminished about one half, when the remainder would be phlogisticated air. I had never observed any *increase* of air in these bottles, though some of them had been used a long time. But having neglected one of them about a year (though the mouth of it had always been kept immersed in water) I casually observed, in the course of last summer, that bubbles of air occasionally issued from it. This surprized me very much, as I imagined it had not been half filled with air; and placing a vessel to receive these bubbles, in about a week or ten days, I got about the quantity of an ounce measure. It contained no fixed air, but was all
strongly

strongly inflammable, and when I analyzed it, it appeared to be of that kind into which alkaline air is converted by heat.

It seems as if the phlogisticated air which is ultimately produced in this process, had been decomposed in these circumstances, so as to form this inflammable air, but on what principle this should take place, I cannot imagine.

In order to make a comparison between the inflammable air produced from this bottle, and that which had been made from alkaline air, I exploded equal measures of each of them with equal quantities of the same dephlogisticated air, the standard of which was 0.3. When I made the explosion with the inflammable air from alkali, the first diminution was to 1.2, and with that from the bottle it was 1.1. Of the former residuum the standard was 1.1; and of the latter 0.98. Neither of them contained any portion of fixed air. On the whole, the resemblance between these two kinds of air was so great, and the difference between

between them so small, that they may be concluded to have had the same origin, and the same constitution.

SECTION XI.

Of the Charcoal of Metals.

MY discovery of the substance which I have termed the *charcoal of metals*, was, as I have observed, quite accidental; a copper tube having been totally dissolved by transmitting through it, when it was red hot, a quantity of spirit of wine in vapour, all the inside of the tube being converted into a black powdery, or friable substance. In order to make farther observations on the nature of this process, I put the copper into an earthen tube, on which I had

I had not found the vapour of spirit of wine to have any action, though itself was decomposed in passing through it, being chiefly converted into inflammable air.

In the first experiment I sent three ounce measures of spirit of wine over two ounces of copper, in a degree of heat that kept it just melted. This was attended with a copious production of such air as would have come from the spirit of wine only. But what surprized me most in the result was, that, though the copper had lost no more than 28 grains of its weight, I actually collected 446 grains of the charcoal; chiefly in the form of powder, though some of it consisted of large flakes, several inches long; having been separated at once from the whole surface of the melted copper. These pieces bore handling without any danger of breaking, and were nearly quite black.

In another experiment I got 508 grains of charcoal from 19 grains of copper. But this was from copper in thin plates, and they were not all converted into perfect charcoal,

charcoal, there being something harder, and therefore partially metallic in the centers of them.

Much of the charcoal was dispersed and lost in the fine black powder with which the air was loaded; and as in what I collected the copper was only about a sixteenth part of the composition, I think I may venture to say that, in reality, it was not more than a twentieth part. In this respect it resembles the charcoal of wood, or of pitcoal, in which the ashes bear a very small proportion to the inflammable air, or phlogiston, of which the bulk of the charcoal consists. This charcoal of copper was also as insoluble in acids as that of wood, and it likewise resembles it in other respects.

When the heat in which this process is conducted is great, the minute division, and volatility of this charcoal, is very extraordinary. Seeing it issue from the end of a tube in a dense black cloud, I endeavoured to collect it in a large glass balloon. But after having given the balloon an uniform,

form, but very thin black coating, not distinguishable in its appearance from *soot*, it issued from the orifice like dense smoke. I then connected with it several adopters, and other vessels, to which it gave a similar coating; and lastly, I plunged the tube out of which it finally issued, very deep in a vessel of water. But still the air came through the water loaded with the same dense smoke, and very little of the matter could be collected*. I was therefore satisfied, that the only way to collect any considerable quantity of it, was to make use of a degree of heat just sufficient to keep the copper red hot, or rather just melting.

Spirit of turpentine I found to answer as well in the production of this charcoal as spirit of wine. In this process I got 120 grains of charcoal from five grains of copper, notwithstanding a very dense black

* The incoercibility of this soot, in the form of vapour, very much resembles that of the dense vapour produced by the decomposition of inflammable and dephlogisticated air, of which an account was given before, both of them passing through water, without being retained by it.

smoke with which the air was charged, and in which, no doubt, a great part of the charcoal was dispersed and lost.

Having procured this new substance, I proceeded to make several experiments upon it; and was much disappointed in finding that in common air it was only melted by the burning lens, and that the heat had not (at least in a short time) any sensible effect upon it. But in dephlogisticated air (as I shall describe more particularly in the section relating to fixed air) it burned rapidly, and converted almost the whole of it into fixed air.

It will not be thought surprizing, that no sensible effect should be produced by heating this substance in inflammable or in alkaline air. But being made red hot in the latter, the air was increased in bulk, and a considerable part of it became inflammable, as it would have been by heating any thing else in it.

Considering this substance as a *charcoal*, and being engaged at the time of my discovery of it, in making steam of water pass

over charcoal of wood, confined in red hot earthen tubes, I treated a quantity of this charcoal in the same manner, and the result was such as might have been expected. A quantity of inflammable air was produced, and there remained a lighter coloured substance, which may be called the *ashes of the metal*. Forty grains of this charcoal were reduced to eighteen by this treatment; and I collected about 200 ounce measures of air, which was very turbid in its first production, and burned with a lambent blue flame.

Having first produced this charcoal from *copper* (owing to my having accidentally found that the spirit of wine acted upon it) I proceeded to try the effect of the same process upon other metals, beginning with *silver*. This metal I found to be affected very much as the copper had been; but though the matter with which the air was charged, was to appearance, equally black with that from the copper, and the vessels in which it was received, so minutely divided, acquired the same black coating, the larger

larger masses of this charcoal of silver were much whiter than those of the copper.

Gold was not at all affected in this process, not being sensibly changed, or diminished in weight by it. At first, indeed, there issued a smoke of a darkish hue, the cause of which I could not discover, but it soon disappeared.

Finding this process to have so remarkable an effect upon copper, and none at all upon gold, I imagined that by this means, it might be possible to separate copper from gold, so that we should be in possession of a new and effectual mode of *assaying*. But I was disappointed in this expectation. I mixed ten grains of copper with an hundred of gold; but the copper was effectually protected by the gold from the action of the spirit of wine, and the whole mass came out of the same weight that it had before it was subjected to the experiment.

I was not able to procure much charcoal from *lead*. Using three ounce measures of spirit of wine, and near four ounces of lead, I got only a small quantity of

a whitish powdery substance, though 58 grains of the lead were missing. But the inside of the glass tube, through which the inflammable air was transmitted, was very black, so that a great part of the lead was probably volatilized, and dispersed, and yet the heat that I used was not great.

Transmitting three ounce measures of spirit of wine over 360 grains of melted *tin*, it was not diminished quite four grains, and the black dust that was collected weighed 26 grains. The air was very dark.

I sent the vapour of two ounce measures of spirit of wine over 960 grains of iron shavings. The result was, that the air was charged with black particles, and the iron was diminished two grains in weight, but I was not able to collect any charcoal. The iron acquired by this means a dark blue colour.

SECTION

SECTION XII.

Of Air extracted from mineral Substances.

HAVING formerly found that manganese and other natural mineral substances, yield a very pure air by extreme heat; it occurred to me that subterraneous fires might maintain themselves by means of the air which they dislodged from such substances as they found in the bowels of the earth. We find that no fire can be kept burning without change of air; and that any other than common, or dephlogisticated air, extinguishes fire as effectually as water. Had subterraneous fires been a simple ignition of wood, coals, or other combustible substances, it would have been highly probable that *air* was necessary for their support. But we find that the greatest heats are produced by mixtures of various substances, independently of any air that

may be in contact with them. This heat excited in the bowels of the earth, from any causes existing there, may be sufficient to volatilize any substance containing phlogiston, and thus produce smoke, flame (which is inflammable air red hot) and every other phenomenon of volcanos. However, as the hypothesis mentioned above led me to try what *kind*, and what *quantity* of air, would be yielded by various mineral substances, in great heats, it may not be useless to recite the experiments; as a knowledge of the results may be useful in other philosophical inquiries.

As the original object of my inquiry respected *volcanic fires*, I gave particular attention to the examination of volcanic substances, especially with a view to ascertain whether a substance which had been in a state of *fusion* will yield air by being heated again or not; in order to distinguish the products of volcanos from other stony matters. Though charcoal, which has been exposed to the most intense heat, will imbibe air from the atmosphere,

atmosphere, and give it out on being heated a second time, yet this is not a substance that can be *fused*; and as this does not appear to be the property of earthly substances, some dependance may perhaps be placed on this test. If so, *basaltes* can hardly be classed among volcanic productions, because they yield more air by heat than any known *lava* that I have met with. But as the results of the experiments that I made both with lavas and basaltes were various, I shall briefly cite them.

Of lava from Iceland $4\frac{1}{2}$ ounces heated in an earthen retort, gave 20 ounce measures of air, of which one half, towards the beginning of the process, was fixed air, and the remainder of the standard of 1.72, extinguishing a candle. In the interstices of this lava, there was a brownish sand, which I could not separate from it.

Of lava from Vesuvius $5\frac{1}{2}$ ounces yielded 30 ounce measures of air, of which the first portion had a slight appearance of fixed air, and the rest was phlogisticated, from the standard of 1.64, to 1.38, which came last.

The retort was broken by the swelling of the mass in cooling.

Another ounce of lava, of the consistence of a hard stone, yielded only $3\frac{1}{2}$ ounce measures of air, chiefly inflammable, which, I suppose, came from the gun-barrel in which this particular experiment was made.

From these experiments it seems probable, that genuine lavas do not give much air; but this will depend upon the degree of heat to which they have been subjected in the subterraneous fire.

It has been much disputed whether basaltés be a volcanic production, or only a crystalization of a mass of matter in a fluid form. The following experiments incline me to the latter opinion.

Seven ounces of basaltés from Scotland, heated in an earthen retort, yielded 104 ounce measures of air, of which the first portion had a slight appearance of fixed air, and was so much phlogisticated as to extinguish a candle, being sometimes of the standard of 1.68.

About two ounces of the *giants causeway* in Ireland yielded 40 ounce measures of air, the first portion of which had a slight appearance of fixed air, and the rest phlogisticated, of the standard of 1.65. It was reduced by fusion to a hard black glass.

Of basaltcs from Scotland, 5 ounces 162 grains, yielded 78 ounce measures of air, of which no part appeared to be fixed air; but was all phlogisticated, sometimes of the standard of 1.7, and towards the last 1.41.

The neighbourhood of Birmingham abounds with a stone which, from its being chiefly got from a village called Rowley, near Dudley, is commonly called *Rowley-rag*. When it is broken, it very much resembles basaltcs, though it is not found in the same regular form. Dr. WITHERING, of this place, has given a most excellent analysis of this substance, which may be seen in a late volume of the Philosophical Transactions. All that I did with respect to it was, to subject it to
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a strong heat in an earthen retort; and from this mode of examination it should seem to be of the same nature with the basalt, whatever that be.

Four ounces of the Rowley-rag yielded 40 ounce measures of air, containing hardly any appearance of fixed air, but was phlogisticated, of the standard of 1.6, and 1.5; the last portion 1.31. It was reduced to a black glassy substance, which broke with a polish, exactly resembling that which remained from the basalt.

The Derbyshire *toadstone*, in its appearance, very much resembles the Rowley-rag, excepting that it is full of white spots, consisting of a calcarious substance. Dr. WITHERING has analysed this, as well as the Rowley-rag, and both from his experiments and mine, they seem to be nearly a-kin to each other. Two ounces and 384 grains of this substance, from which the calcarious part had been dissolved by spirit of nitre, yielded 60 ounce measures of air, the first portion of which contained a little
fixed

fixed air, perhaps from some unperceived remains of the calcarious matter. The rest was phlogisticated, of the standard of 1.7.

In another experiment, an ounce and a quarter of this substance, from which the calcarious part had been extracted first by oil of vitriol, and then by spirit of nitre, yielded 40 ounce measures of air, of the same quality of that in the former process. There remained from both of them a black glassy matter, which seemed to be very liquid when it was hot, as part of it had boiled up into the neck of the retort.

Granite, like basaltes, has been thought by some to be the product of volcanos, and by others to be a crystallization from a liquid state. The latter is the opinion favoured by these experiments. From about an ounce and an half of this substance I got 20 ounce measures of air, the first portion of which contained a little fixed air, but the rest was phlogisticated, from 1.7 to 1.28, which is nearly the standard of common air; but the heat was very intense, as the substance was reduced to a glass.

Again,

Again, 5 ounces and 252 grains of a blue granite yielded 70 ounce measures of air, of the same quality with the preceding. It was also reduced to a firm uniform glassy substance of a dark-brown colour. Upon the whole, therefore, it seems probable, that the origin of granite is similar to that of basaltes.

In Cornwall there is a substance called *elvain*, the natural history of which is very like that of granite, and the result of my experiments upon it shews that they are of the same nature. There is a black and white kind of *elvain*.

Of the black *elvain* one ounce and 288 grains yielded 25 ounce measures of air, the first portion of which contained a little fixed air, and the rest was phlogisticated, of the standard of 1.54. It was melted into a brownish black mass.

Of the white *elvain* one ounce and 384 grains yielded 30 ounce measures of air, of the same quality with the preceding. It was converted into a very porous substance, exactly resembling a pumice stone, but much harder.

The substance called *groan clay* is said to be formed by the decomposition of granite. Of this substance one ounce and 72 grains yielded 32 ounce measures of air, containing no sensible quantity of fixed air, but all phlogisticated, of the standard of 1.62 and 1.33. After the experiment this matter was easily shaken out of the retort, and was not sensibly changed in its appearance.

Such were the experiments that I made with substances that are, or are supposed to be, volcanic. Of those which are certainly *not volcanic*, but which may come in the way of volcanic fires, I found those into which the vitriolic acid enters to yield the greatest quantity of pure air; but by no means sufficient to keep alive such fires as we make on the surface of the earth.

From seven ounces of *gypsum*, which I kept in a strong heat twelve hours, I got 230 ounce measures of air, the greatest part of which would have extinguished a candle; the most phlogisticated being of the standard of 1.8, but it was afterwards much purer; and at the last considerably dephlogisticated;

gified; for with two equal measures of nitrous air, the test was 1.3. The air was very turbid as it was produced, and the purest of all came rapidly, at the end of the process. It is possible that, with a stronger heat, more and purer air might have been procured. The substance was reduced to a hard mass, yellow next to the retort, but in the middle very white.

In this place I shall just mention the air that I got from *alum*, as a substance containing vitriolic acid, though not likely to be found in the bowels of the earth. From one ounce of *calcined alum*, very white and clean, I got 60 ounce measures of air, without any fixed air, or the least imaginable, and so pure, that with two equal measures of nitrous air, the test was 1.4. Still the residuum had an acid taste, so that with more heat, it is probable that more, and purer air, would have been procured.

The stones which I found to furnish the greatest quantity of air, though not the purest, were those of the *schistus* kind, which are found in great quantities in many

many mountainous countries ; and after being subjected to a very great heat, have the nearest resemblance to the generality of lavas of any substance on which I have yet made the experiment.

From four ounces of a *blue slate* I got 320 ounce measures of air, a very small portion of which was fixed air, and the greatest part of the rest (the whole, I believe, except about 20 ounce measures) so impure, that the standard was generally 1.8. Towards the last it was 1.5, and the last of all 1.35 ; so that a candle would just have burned in it. The air was very turbid, and had a very strong smell. The substance was perfectly vitrified, and quite black, exactly resembling lava. It then weighed, as nearly as I could guess (for in the fusion it had adhered closely to the retort) 3 ounces and 288 grains.

From eight ounces of another kind of schistus, I got 70 ounce measures of air, of the same quality with that in the preceding experiment ; and it was melted into a black mass, harder than the former, so as to make a still more perfect lava.

Oil of vitriol has been supposed to enter into the composition of clay. From four ounces of it I got 26 ounce measures of air, in every portion of which one-tenth was fixed air, and the rest of the standard of 1.72, 1.52, and at last 1.44.

Putting oil of vitriol to this clay, it yielded much more air, and of a better quality. Two ounces of the clay, moistened with this acid, gave 216 ounce measures of air, exceedingly turbid, containing very little fixed air, and the rest of the standard of 1.5, 1.7, 1.58, in the order in which they are here put down; but the last portion was 1.08, and was considerably dephlogisticated.

A quantity of fine white clay from the *Apalachian mountains* gave air of the same kind at the beginning of the process with common clay, but the retort being cracked, the experiment was interrupted.

Nothing in the form of a stone yields so much air as *lime stone*, and this is by no means all fixed air, as I believe has generally been supposed. For a very great proportion of it is more or less phlogisticated, and

and the last portions often tolerably pure, so that a candle would nearly burn in it.

From four ounces of white *crystals of lime stone* I got 830 ounce measures of air, the first portion of which had only one-fourth of fixed air, but in the course of the experiment it varied, being once three-fourths, then one-half, and at the last one-third. The standard of the residuum was never better than 1.56, nor worse than 1.66.

From five ounces and a half of lime stone of an excellent kind, I got in all 1160 ounce measures of air. Of this one-tenth only was phlogisticated, and the rest fixed, but the last portion of all was half phlogisticated.

From seven ounces of a transparent substance, found in a stone in the neighbourhood of Oxford, which is chiefly calcareous, I got 1280 ounce measures of air, of which about one-third of the whole was fixed air. The standard of the residuum was at first 1.55, and afterwards 1.44.

From six ounces of a *blue stone*, found in the neighbourhood of Stratford, I got 1030 ounce measures of air, of which, till near

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the end of the process, about one-half was fixed air, and at the last about one-fourth. The standard of the remainder was about 1.6.

From three ounces of *chalk* I got 630 ounce measures of air, of which at the first one-fourth was fixed air, then almost two-thirds, then something more than one-half, and again a little more than a third. The standard of the residuum was from 1.66 to 1.34.

Iron ores may be pretty well distinguished by the quality of the air that they yield by heat, as well as by their weight, and external appearance. The residuum of the air from other stony substances, after the fixed air is separated from it, I have always found to be phlogisticated, but that from iron ore is inflammable.

Three ounces and one-half of *white spathe iron ore* yielded 560 ounce measures of air, of which at the first one-third was fixed air, then only something more than one-half, and again at the last a third. The standard of the residuum was about 1.7, and inflammable. The substance was reduced to one

hard mass, and the bottom of the earthen retort was melted along with it.

I tried one iron ore that was of a *light* colour, and another of a *darker*. Six ounces of the lighter coloured ore yielded 750 ounce measures of air, of which at the first two thirds were fixed air, then only a little more than a half, and at the last a fifth. The residuum of the middle portions of this air only was inflammable, that of the rest phlogisticated; the standard of it about 1.7. It was reduced to a hard black slag full of cavities.

Four ounces of the dark coloured ore yielded 510 ounce measures of air, the quality of which varied very much, like that in the preceding experiment, and the air was not more strongly inflammable.

There is, I believe, some iron in the substance that is called *black lead* (*molybdena*) and therefore I mention the experiment that I made with it in this place. From half an ounce of it I got 25 ounce measures of air, of the standard of 1.6, and 1.42. I have

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have no note of any part of its being fixed, or inflammable. It had lost only 18 grains in weight. From eight ounces and a half of another kind of black lead, I got 16 ounce measures of air, one-fifteenth, or one-twentieth of which was fixed air, and the rest inflammable, burning with a blue flame.

I did not pursue these experiments on ores to any great extent; but having some *stream tin*, I found that 110 grains of it, gave 20 ounce measures of air, a small portion of which was fixed air, and the remainder of the standard of 1.44, and at last 1.34.

Of the air extracted from *manganese* in glass retorts, I gave an account in a former volume. I now found that seven ounces of it, with a stronger heat, in an earthen retort, yielded 110 ounce measures of air, of which at the first, a small portion was fixed air, and of the rest the standard was in different portions of it 1.54, 1.25, and 1.05; so that the bulk of it was not perhaps better than common air. The substance was con-

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creted into a hard mass, but was loose from the retort.

At another time, from eight ounces of another quantity of manganese, I got 1200 ounce measures of air, of which one third was fixed air, and the rest inflammable, burning with a blue flame: To what circumstance this remarkable difference in the quantity and quality of the air, from what was called the same substance, was owing I cannot tell. Something else besides manganese, as I suspect, must have been mixed with it in this experiment.

I shall conclude this section with an account of the examination of some other substances, of which the results are not always of much consequence.

From two ounces and one-fifth of *searites*, I got 43 ounce measures of air, which had the slightest appearance of containing fixed air. The remainder was thoroughly phlogisticated, except that, at the last, it was of the standard of 1.65. It came out of the retort a yellow mass, but powdery, as it was put into it.

Two ounces of *terra ponderosa*, gave 26 ounce measures of air, without any mixture of fixed air, the standard of it 1.62, 1.42, and 1.29. The substance was concreted into one mass, but was easily broken by shaking the retort, and then it did not appear to be changed in its external appearance.

Two ounces of *black wood* from Derbyshire, yielded 80 ounce measures of air, no part of which was fixed air, but all better than common air, the standard of it being 1.05. This circumstance may help to account for this substance taking fire, and burning as it does, when it is mixed with linseed oil. For if by any means it is so far heated, as to give out its pure air, this must assist the combustion; and the chymical attraction between the phlogiston in the oil, and the dephlogisticated matter in the wood may, without its assuming the form of air, be the cause of the mass becoming hot.

Seven ounces of *fluor* gave eight ounces of air, a small proportion of which was fixed air,

air, and the rest of the standard of 1.45. It was melted into a hard mass. Six ounces of white fluor yielded in all ten ounce measures of air, of which the slightest portion imaginable was fixed air, the rest of the standard of 1.34, and 1.3. In this experiment the bottom of the retort was quite dissolved. N. B. there was no appearance of fluor acid in the water in which this air was received, and the melted mass gave fluor acid air with oil of vitriol.

From four ounces of a kind of *sand-stone*, I got 75 ounce measures of air, a small portion of which was fixed air, the standard of the rest, for the most part, 1.75, and at the last 1.35. When taken out of the retort, it weighed three ounces and three-fourths. That part of it which was next to the bottom of the retort was whiter than the rest, but very hard, adhering to it; and, what was pretty remarkable, the remainder had acquired just as firm a texture as it had before it was pounded for the purpose of the experiment.

Five ounces of a fine *white sand-stone*, yielded about ten ounce measures of air, containing a little fixed air, and the rest of the standard of 1.6. This also, was again reduced to a stone quite as compact as it had been before it was pounded.

Six ounces of another *sand-stone* yielded 102 ounce measures of air, of which a very small portion was fixed air, and the rest of the standard of 1.57, and 1.35. This also was reduced to a hard dark coloured stone, having separated itself from the retort about a quarter of an inch, except at the bottom where it adhered to it.

From one ounce and 175 grains of *belemnite*, I got 320 ounce measures of air, of which at the first one sixteenth was fixed air, the rest of the standard of 1.75, 1.55. All the air came while the heat was very moderate.

From four ounces of *crystals of quartz*, I got 25 ounce measures of air, a very small portion of which was fixed air, the rest being of the standard of 1.8, and 1.44.

From seven ounces of a *granulated quartz*, I got about ten ounce measures of air, containing

taining a little fixed air, and the rest of the standard of 1.42. It came out of the retort a loose friable substance, weighing six ounces 290 grains. The retort was cracked, or more air would probably have been procured.

From one ounce and 84 grains of *mica*, I got twelve ounce measures of air, of which no part was fixed air, but of the standard of 1.4, and 1.35.

From 120 grains of *talc*, I got a quantity of air, but the retort being cracked at the beginning of the process, I took no account of the quantity. Part of it was evidently fixed air, and the rest of the standard of 1.4, and a candle burned in it. The substance was reduced to a dark hard cinder, adhering to the retort.

From four ounces 355 grains of *crystallized glass*, in the form of a whitish stone, I got twelve ounce measures of air, which contained no fixed air, and of the standard of 1.42, 1.36, and 1.31. Perhaps I used a greater degree of heat than the glass had been

been subjected to before. Otherwise this experiment might help to account for lava giving some quantity of air, though it had been in a state of fusion, having afterwards crystalized, like this glass.

The last experiment that I shall mention was made with *pit coal*. Three ounces of such coal as we have at Birmingham, gave 700 ounce measures of air, of which I could not be sure that any portion was fixed air. It was all inflammable, the first portion of it burning with a white lambent flame, and the last with a blue one.

SECTION

SECTION XIII.

Experiments on Charcoal.

THE examination of *charcoal* is now considered as an object of peculiar importance in chymistry; and ever since I had discovered it to be one of the best conductors of electricity I have given particular attention to it. The most important of the experiments that I have made respecting it since my former publications, will be found in the section relating to phlogiston, in which an account is given of the decomposition of it by steam in a state of ignition. In this section I shall comprize a variety of miscellaneous observations, some of more, and others of less importance. Some of them also are repetitions of former experiments, but made with a better apparatus, and with an attention to more circumstances.

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The quantity of air to be expelled by heat from dry wood I have frequently estimated; but having now, by the assistance of Mr. WEDGWOOD, the advantage of doing every thing of this kind in very compact earthen retorts, which themselves give no air, it may be worth while to mention, that from five ounces of dry oak I got 650 ounce measures of air, of which about one-half, nearly a third, that came at first was fixed air, the remainder being inflammable, and the last portions wholly so.

The property that charcoal has of absorbing air is a remarkable circumstance, first distinctly observed by the Abbe FONTANA; but still there are several particulars relating to this experiment, such as the *time* in which the air is imbibed, and the quality of it when it is again expelled by heat, &c. that are not undeserving of notice. And though I have not pursued this subject with much regularity, I have occasionally, and at different times, made observations of this kind, of which I shall here give an account; proposing to resume
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the experiments, and to pursue them farther.

From 789 grains of charcoal, from which all air had been expelled, and which had been exposed to the atmosphere, I got 39 ounce measures of air, no part of which was fixed air, but all phlogisticated, extinguishing a candle; being of the standard of 1.7. Probably pure air had been imbibed in preference to any any other; because when it has been made to imbibe dephlogisticated air only, it comes out again partially phlogisticated. I found, however, considerable varieties in the quality of air emitted by charcoal, after being exposed to the open air, as well as other circumstances of some consequence relating to the experiment.

From 680 grains of charcoal, which had been heated four times before, I got 40 ounce measures of air, of which the slightest portion imaginable was fixed air. Of the rest, towards the middle of the process, the standard was 1.48, and the last, 1.52. The next day, without changing the retort, or moving it from its place, having only left it

it with its mouth open, to give it an opportunity of attracting more air, I heated it again, and got about two ounce measures of air, of the standard of 1.5. This was much less than I had expected. I then tied a bladder to the mouth of the retort the moment it ceased to give air. But though the charcoal was shaken out of the retort into the bladder when it was cold, very little air had been absorbed by it. It then weighed 560 grains. But having been thus exposed to the open air, though for a short space of time, on being again immediately subjected to heat, it gave 50 ounce measures of air, the standard of which varied in different periods of the process, in the following order: 1.54, 1.58, 1.7, 1.6, and about one-twentieth of the whole was fixed air. Why this same charcoal should give 50 ounce measures of air now, and only 40 before, I cannot tell. Probably a little moisture had been attracted by it.

Being willing to ascertain the weight that was gradually gained by charcoal in consequence of being exposed to the open
air,

air, on the 4th of September I left in an open dish charcoal fresh made from dry oak, weighing 364 grains.

The next day it weighed 390

Two or three days after, 397

The 24th of Oct. following 419

The 26th of April 421

It appears, therefore, that charcoal fresh made, only absorbs about half as much air on its first exposure to the atmosphere as it does in a course of time afterwards.

Judging that this charcoal would not now acquire any more weight, I subjected it to heat in an earthen retort; and having got from it a quantity of air that was considerably phlogisticated, found that it weighed 312 grains, but the retort appeared to be cracked.

Having left the same charcoal exposed to the open air a whole year, I weighed it again, and found it to be 371 grains. That this charcoal should be reduced to less weight than it had been at the beginning of the process, I could not account for at that time; but I now do it by supposing

that, together with *air*, some *moisture* had been imbibed; and this would help to decompose the charcoal, when it was subjected to the fire again, as is explained in the section relating to phlogiston. It may, however, be determined whether the air expelled from charcoal by heat be the air which it had imbibed, or that which was formed by the decomposition of the charcoal by means of water. For this will be inflammable air, whereas the other, as appears by these experiments, will be partially phlogisticated.

Having gone through another process of exposing charcoal to air, and then expelling air from it by heat, I shall here note the particulars of it.

From two ounces of pounded charcoal, on the 20th of January, I expelled, by means of a strong heat, 336 ounce measures of air, and weighing it immediately afterwards, found it to be 756 grains. On the 23d it weighed 817 grains, after being exposed on a plate, so as to lie about half an inch in depth. The air expelled from it

was

was about one-tenth fixed air. This charcoal I exposed to the fire several times, the last time on the 28th of June in the year following, immediately after which it weighed 711 grains. Some of the air that I got from this charcoal was inflammable, burning with a lambent blue flame, which shews that moisture had been imbibed by the charcoal.

In a former publication I observed, that when charcoal has imbibed air, it will give it out again, at least in part, on being plunged in water, as well as by being exposed to heat. I again plunged into water pieces of charcoal, both perfectly and imperfectly made, after having been some days exposed to the open air, and found that the air they gave out in this way was in both cases common air. It is evident, therefore, that the degree of phlogistication in the air, expelled by heat, is owing to the decomposition of the charcoal.

Having repeated the Abbe FONTANA's experiments, by introducing hot charcoal through mercury, into vessels containing

different kinds of air, and being willing to recover by distillation the mercury that had been imbibed by the different pieces of charcoal, I at the same time took some notice of the *air* that came from them. The quantity of air was very considerable, but I took no exact account of it. With respect to its quality, it was partially phlogisticated, the standard of it being about 1.6, though the last that came was inflammable.

In my experiments with respect to phlogiston, I made several on the decomposition of charcoal in the sun, the general results of which are there mentioned; but as the particulars were not many, I shall here recite them. It is only to be observed, that at the time that I made them I was under a mistake with respect to the origin of the air I procured, imagining it to proceed wholly from the charcoal; whereas I afterwards found that a degree of moisture, to which I had not then attended, was necessary to the formation of that air. These experiments were all made by the heat of the sun in vacuo.

Four grains of charcoal treated in this manner, yielded twenty ounce measures of air, all inflammable, except that it barely made lime water a little turbid; but without any diminution that I could measure. At another time four grains of charcoal gave 24 ounce measures of air, no part of which was fixed air.

I entirely dispersed one grain of charcoal in vacuo, and it gave six ounce measures of air, without the slightest appearance of its containing any fixed air. This charcoal had been long exposed to the open air, and on that account would give out more air than it otherwise would have done.

Four grains and an half of charcoal gave 22 ounce measures and an half of air; and lastly, three grains and three quarters gave 23 ounce measures and an half of air, without the least portion of fixed air in it.

I shall close this account with an experiment, in which I proceeded to take the specific gravity of the air which I got in these processes, without at the time drawing a very obvious conclusion from it. From

about $2\frac{1}{2}$ grains of charcoal of oak, I got $15\frac{1}{2}$ ounce measures of inflammable air, no part of which was fixed air; and weighing this air, I found that 20 ounce measures of it weighed four grains and a quarter less than the same bulk of common air. According to this proportion the 15 grains and an half of inflammable air ought to have weighed 5.96 grains, which is much more than the weight of the charcoal. But though I made this very observation at the time, I did not then infer, that *water* must enter into the composition of this air, having no suspicion that the water at the bottom of the receiver, several inches below the place on which the charcoal was exposed to the heat of the lens, could be attracted by it. Had not subsequent experiments shewn me the real nature of this inflammable air, this experiment must have remained inexplicable by me. At the time I imagined, I believe, that the additional weight of the air was owing to the extraneous *water* which it had imbibed in being transferred from one vessel to another.

Having

Having made the preceding experiments with charcoal from *wood*, I made a similar one with that from *pit-coal*, and I found that a piece of it, heated in vacuo, yielded four ounce measures of air, after having lost something less than a grain in weight. The air had in it a small quantity of fixed air, but the rest was all inflammable. The charcoal had been a long time exposed to the common air.

After the preceding experiments with charcoal, it occurred to me to vary them by making use of *soot*; but I was much surprized indeed, to find that it was a substance very different from charcoal, with which at first sight it might seem right to class it; for it contained a portion of pure air. A quantity of it being put into an earthen retort, and exposed to a strong heat, yielded air so pure, that with equal quantities of nitrous air, the test was 0.5, a degree of purity far exceeding that of common air. There was, however, some inflammable air mixed with it, which made it burn with a slight blue flame.

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Taking some of this foot from which air had been expelled by heat, I again exposed it to the heat of a burning lens in vacuo, and from a grain and a half of it got six ounce measures of air, all inflammable, and burning with a blue flame, without any fixed air in it. It was, therefore, after the former experiment, a true *charcoal*, but not before.

SECTION XIV.

Of the constituent Principles of Fixed Air.

IN the paper which I presented to the Royal Society on the subject of phlogiston, I gave a general account of some experiments, which seemed to prove the composition of fixed air from dephlogisticated air and phlogiston. I shall now give the particulars of those experiments, and also
others

others which seem to lead to the same conclusion.

The experiment upon which I then laid the greatest stress, as appearing to me the most conclusive, was that with *red precipitate* and *iron filings*, the former of which yields only dephlogisticated, and the latter only inflammable air. But the first experiment that I made of the kind was with *charcoal* and red precipitate, the charcoal being made with so great a degree of heat, that no fixed air could be expelled from it, not even when it was wholly dispersed by the heat of the sun in vacuo. This experiment is certainly, however, not so conclusive as the former; because, since dry wood and imperfectly made charcoal yield fixed air, it may be said that all the *elements* of this kind of air were contained in the most perfect charcoal. And though this substance alone will not, even with the assistance of water, give fixed air, it might be said, that this might be effected by its treatment with other substances, without their imparting any thing to it; especially

as

as the inflammable air which is procured from charcoal by means of water appears to contain fixed air, when decomposed with dephlogisticated air. I think, however, that I have in a former section, contrary to my own first supposition, proved that this fixed air is really a composition of phlogiston contained in charcoal, and of the dephlogisticated air with which it was inflamed, the charcoal contributing nothing to it beside its phlogiston. In this place I shall only recite the facts concerning the production of great quantities of fixed air from perfect charcoal and red precipitate.

In order to expel all fixed air, I made a quantity of perfect charcoal from dry oak; and while it was hot I pounded it, and immediately mixing four measures of it with one of red precipitate, and putting them into an earthen retort, I presently got, in no greater a degree of heat than was necessary to revive the mercury, a large quantity of air, half of which was fixed air. Afterwards the proportion of fixed air was less, and towards the conclusion of the experiment

periment there came no fixed air at all. This residuum was a little better at the first than at the last, when it was of the standard of 1.5.

As this air contained a greater portion of phlogisticated air than the common air of the atmosphere, and no spirit of nitre, or any thing that could yield spirit of nitre, was concerned in the experiment, it should seem that phlogisticated air may be composed of phlogiston and dephlogisticated air; though this composition, according to the very capital discovery of Mr. CAVENDISH, may be reduced to spirit of nitre, or rather become one element in the composition of that acid.

In another experiment I hit upon a better proportion of the charcoal and red precipitate for making pure fixed air. For mixing one ounce of red precipitate (which all chymists, I believe, are agreed to be the same thing with *precipitate per se*) and one ounce of perfect charcoal, fresh from the retort in which it was made; and putting them into a coated glass vessel, I procured
from

from the mixture, by heat, about 30 ounce measures of air, the whole of which was the purest fixed air, leaving only about one-fortieth part not absorbed by water, and this not inflammable, but of the standard of 1.7, or almost perfectly phlogisticated.

This experiment made me recollect those which I had formerly made with charcoal heated in nitrous acid, in which I had always procured a quantity of fixed air. I therefore repeated the experiment with some of the same charcoal which I had used in the preceding experiment, on the goodness of which I could depend; and I found that, when it was heated in the acid, in a glass phial with a ground stopper, it gave air, one-fifth of which was fixed air. At another time I got air in this process, one-half of which was fixed air. To the formation of this air, I presume, that the phlogiston from the charcoal and the dephlogisticated air, which is known to be produced by heating nitrous acid, must have contributed.

Being then apprized of the objection that might be made to the use of charcoal, as,
notwith-

notwithstanding the great heat with which it was made, containing at least the elements of fixed air, I made use of *iron*, to which no such objection could be made; and mixing an ounce of the red precipitate with an ounce of iron filings, and then heating them in a coated glass retort, I got 20 ounce measures of air, of which one-seventh remained unabsoꝛbed by water. The residuum was of the standard of 1.52, but slightly inflammable.

Again, from half an ounce of red precipitate, and half an ounce of iron filings, I got 26 ounce measures of air, of which the first part was pretty pure fixed air; but afterwards one-tenth of it remained unabsoꝛbed by water. Then, increasing the proportion of iron, I mixed one ounce of red precipitate with two ounces of iron filings, and got about 40 ounce measures of air, of the first portions of which only one-twentieth was unabsoꝛbed by water, though towards the conclusion of the process this residuum was greater. In this process I got, in the whole, 36 ounce measures

tures of pure fixed air, completely absorbed by water, besides what was absorbed both in the first reception of the air (which was in vessels containing water) and afterwards in transferring this air into those vessels in which the quantity of it was noted, the whole of which I suppose might be about four measures more. Examining the first residuum of this process by nitrous air, the standard of it was 1.6, and afterwards 1.7.

Having heard that it was objected to this experiment, that iron contains a quantity of *plumbago*, and that the fixed air which I procured might come from that ingredient in it (though the quantity was certainly much too great to be accounted for in that way) I made use of other metals, to which no such objection could be made, viz. *brass* and *zinc*, and with the same result.

With two ounces of brass dust I mixed one ounce of red precipitate, and in a coated glass retort I got from it a quantity of air, two-thirds of which was fixed air. The standard of the residuum was 0.6; so that there had been too great a proportion of the
red

red precipitate. But fixed air was produced in a quantity abundantly sufficient for my purpose.

In a coated glass retort, I put a mixture of one ounce of red precipitate and one ounce of filings of *zinc*, and got some air, part of which was clearly fixed air; but the retort very soon cracking put an end to the experiment, and I did not think it necessary to repeat it. I imagine, however, that it will only be at the beginning of this process that much fixed air can be procured, unless more precaution be used in conducting it. For the neck of the retort breaking quite off, there issued from it a strong flame, which evidently arose from the burning of the zinc in the dephlogisticated air from the precipitate.

As turbeth mineral gives dephlogisticated air, as well as red precipitate, I mixed this substance with iron filings, and had a similar result, when I heated them together in an earthen retort. One ounce of the turbeth mineral with two ounces of iron filings, yielded about sixteen ounce measures of air, of which

which about one-third was fixed air, and the rest of the standard of 1.5.

Another experiment which seems to prove the formation of fixed air from phlogiston and dephlogisticated air, is the expulsion of it from that *black powder* which is formed by the union of lead and mercury. This powder, I have observed, can only be made in pure air, which is no doubt absorbed by the metals; and this being again expelled by heat, together with the phlogiston which had belonged to the lead, is that, I presume, which forms the fixed air that is found in this process.

When I began to make observations on this black powder, I mentioned my having expelled some fixed air from it. This was from such powder as I had found ready made; and therefore, not knowing with certainty what the composition of it was, I dissolved one ounce of lead in pure mercury, and then expelled it again in the form of this black powder, which, when the running mercury was pretty carefully pressed out of it, weighed about twelve ounces.

Then

Then exposing it to heat, in a coated glass retort, I got from it about 20 ounce measures of air, making allowance for the quantity of fixed air, which, as I supposed, might have been absorbed by the water, in receiving and transferring the air before any account was taken of the quantity of it. Of this air about one-thirtieth part only was not absorbed by water. The residuum I did not examine. I must however observe, that in general, besides the fixed air, I obtained a considerable quantity of the purest dephlogisticated air, from this black powder.

In making the black powder that was used in the preceding experiment, I occasionally changed the air in the phial, in which I shook the mercury, by blowing into it, sometimes with a pair of bellows, and sometimes with my mouth; and as it was suggested that this might have supplied the fixed air which I afterwards found in the black powder, I dissolved two ounces of lead in mercury, and got the black powder without blowing into the vessel at all, only changing the air so much oftner as was

then necessary. From six ounces of the black powder thus carefully prepared, I expelled $4\frac{1}{2}$ ounce measures of air, of which one and a half was pure fixed air. This was sufficient to satisfy me that *some* fixed air is certainly procured in this process. The residuum of this fixed air was of the standard of 1.7, or 1.8. I did not at this time get from this powder all the air it would have yielded.

Being now satisfied that there was no occasion to prepare this black powder with the precaution mentioned above, I repeated the experiment with ten ounces of it prepared in the readier method which I had used before, with a view to examine the residuum of the air, when the fixed air should be separated from it. The produce of air was in all about 23 ounce measures, which I received in four portions of five ounce measures each, and another containing the remainder. All these portions I examined separately, observing the proportion of residuum in each of them, and the quality, as measured by my usual standard,

standard, and the result was as follows : Of the first portion there remained one-fourth, of the standard of 1.6 ; of the second one third, of the standard of 1.44 ; of the third one-half, of the standard of 0.8 ; and of the fourth three-fourths, of the same quality with the preceding.

In the last portion the residuum was one-half of the whole, and that I found to be so pure, that, mixing it with two equal quantities of nitrous air, the standard of it was 0.63 ; so that the quality of these residuums was continually purer, till at the last it was pretty highly dephlogisticated.

It may be inferred from both these courses of experiments, that fixed air consists not of inflammable air (which I suppose necessarily contains water) but of pure phlogiston, and dephlogisticated air. In the experiments with the red precipitate and iron, no water at all is concerned, unless either the iron itself contain some, or the mercury, or dephlogisticated air : since when the red precipitate is decomposed by itself, nothing is produced

produced besides mercury and dephlogisticated air, without any water. The experiment with the black powder will equally authorize the same conclusion, as neither the lead, the mercury, nor the pure air that combines with them, can be said to contain any water.

It may be worth while to observe that, in making this black powder, the phial in which the lead and mercury are shaken, grows very warm, as the coagulum of the two metals (from which the black powder is pressed) begins to form, and that in squeezing this coagulum in a cloth (which is the readiest method of separating the running mercury from the black powder) it suddenly becomes so hot, that I could sometimes hardly bear to handle it.

For the information of those who may wish to repeat these experiments, I would observe, that in using as much mercury as I can conveniently shake, in a phial containing about three pints of water, I have got, with four hundred concussions of the amal-

gum (blowing into the phial with a pair of bellows after every hundred concussions) near eight ounces of the black powder.

While I am upon the subject of this black powder, I shall observe, that it occurred to me to mix with it more matter containing phlogiston, in order to see what change that would make in the residuum of the produce of air.

From four ounces of the black powder mixed with two ounces of iron filings, heated in an earthen retort, I expelled 54 ounce measures of air, of which not more than four ounce measures were fixed air, and the residuum, examined at different times, was of the standards of 1.3, and 1.44; but the greater part of it was of 1.52, so that there was a considerable production of inflammable air from the iron. In this experiment, I raised the heat very gradually, till I had got one third of the produce of air. This I did from an idea that this moderate heat might increase the quantity of the fixed air, but it did

not appear to make any difference in this respect.

Then varying the proportion of the ingredients, I mixed 20 ounces of the black powder with only one ounce of iron filings, and receiving the air in three portions, observed as follows. The first portion, which contained six ounce measures, had a residuum of 3.5, of the standard of 1.6. The second, which was one ounce measure, had a residuum of 0.12, of the standard of 1.7; and the third portion, which was only one ounce measure, had a residuum of 0.12, of the standard of 1.7. Whether this was the whole of the produce of air, I do not recollect.

In order to try more fully the effect of different degrees of heat, I repeated the process with the black powder, only determining to suspend the process in the middle of the produce of air. Accordingly I heated two ounces of the black powder in a porcelain vessel; when I observed that some portions of the produce contained about one
half

half fixed air, and that this proportion kept growing less and less, till the produce consisted of nothing but the purest dephlogisticated air, the standard of it being, with two equal measures of nitrous air, 0.2. I then let the vessel cool, and observed that, on resuming the experiment, the air came with the same purity to the last.

Examining the residuum in the retort, I found half an ounce of red powder, the colour of which could hardly be distinguished from that of precipitate *per se*. So that, no doubt, the mercury had been converted into it, and this very pure air was probably that which came from the precipitate as it was reviving. In this way, therefore, it would be easy to make this precipitate in large quantities, could a method be found of separating it from the *red lead*, with which it is, in this process, necessarily mixed.

In the preceding experiment it will have been observed, that, at first, the residuum was considerably phlogisticated, but at the last remarkably pure. An accident

in a subsequent experiment I once thought had discovered the cause of this difference. In the middle of one of the processes, in which I was using the black powder only, heating it in a glass vessel, a quantity of water was drawn up through the tube that communicated with the recipient, and got into the vessel that contained the black powder; and in all the remainder of that process, the residuum of the air was no better than about the standard of 1.7. Water came over along with this air to the very last, though the bottom of the vessel was red hot. When the process was over, the matter taken out of the vessel was still moist, and of a dark grey colour.

On this I made a paste of the powder with water, and drying it a little, immediately repeated the experiment with it; but I found no sensible difference between the substance in this state, and that which had not been wetted. Four ounces of it yielded 120 ounce measures of air, of which about 12 were pure fixed air, completely absorbed by water, and the rest highly dephlogisticated

ed. However, in one process of this kind, from two ounces and a half of this powder, which had been moistened and dried again, I got 70 ounce measures of air, of which only a very small part was fixed air, and the residuum was by no means pure dephlogisticated air. For with two equal measures of nitreous air, the standard was 1.2 and 1.3. At other times also I have had much less fixed air from this black powder when it had not been wetted, than in several of the instances above mentioned; and I have not as yet been able to discover the circumstance on which the production of it in a greater or less quantity depends.

In the preceding processes with this black powder, I always got from it more or less of fixed air. But thinking to produce more of it by heating this substance with a burning lens in dephlogisticated air, I was surprised to find, that I only increased the quantity of dephlogisticated air in the vessel, and produced no fixed air at all. Whence this remarkable difference could arise, I do not pretend to say. It will be seen,

seen, that, in this process with inflammable air, I found it to be a matter of indifference whether I used this black powder or the red precipitate; both of them equally imbibing inflammable air, without producing either water or fixed air.

Another decisive proof of the generation of fixed air from phlogiston and dephlogisticated air, is the constant production of it when iron is melted in dephlogisticated air over mercury, by means of a burning lens. This experiment being a very pleasing one, I repeated it very often; and as it is on too small a scale to admit of great exactness, I shall mention the results of several of them, observing, in the first place, that no water is produced in this process.

At one time I observed, that when two-thirds of the dephlogisticated air disappeared, one-ninth of the remainder was fixed air. At another time seven ounce measures of dephlogisticated air were reduced to two, of which more than one-third was fixed air. Again, six ounce measures and a half were reduced to half an ounce measure, and one half of this was fixed air. I repeated the

the experiment again, at the same time, and had the very same result.

In all these experiments it cannot be doubted but that the greater part of the dephlogisticated air incorporates with the iron, converting it into a *scale*, or *finery cinder*, being the very same substance with that which is produced by transmitting steam over iron when it is red hot; but at the same time some phlogiston must be expelled from the iron, and unite with the dephlogisticated air in the vessel, in order to form the fixed air that is found in it; as in other cases it unites with *water*, and makes inflammable air.

Perhaps as decisive a proof as any of the real production of fixed air from phlogiston and dephlogisticated air, may be drawn from the experiments in which I always found a quantity of it when I burned sulphur in dephlogisticated air. In one of those experiments to which I gave more particular attention, six ounce measures and an half of the dephlogisticated air were reduced to about two ounce measures, and one-fifth of this

this was fixed air. Much vitriolic acid air had been produced in this process: For, before I admitted any water to it, the fix and an half ounce measures were only reduced to fix. When both the vitriolic acid and the fixed air were absorbed by water, the remainder was very pure dephlogisticated air, the standard of it being 0.3.

I had always concluded, that no fixed air could be produced by the decomposition of inflammable air, which had been procured by means of the mineral acids, because I had not been able to do it with that which I had got by means of the vitriolic acid; but I learned from Mr. METHERIE (in whose ingenious treatise on air I cannot help wishing he had indulged a little less to theory) that this is peculiar to the vitriolic acid, the remains of which, diffused through the inflammable air procured by it, he conjectures, may decompose the fixed air actually produced in the process. See his Treatise, p. 110. For, as I have hinted before, when the inflammable air is produced from iron, by means of spirit of salt, there

there is a very perceivable quantity of fixed air, when it is united with dephlogisticated air. When I decomposed these two kinds of air in equal quantities, they were reduced to about 0.5 of a measure, and of this not more than about one fortieth part was fixed air. This experiment, ought, however, to be added to the other proofs of fixed air being produced by the union of dephlogisticated air and phlogiston.

The last instance of the generation of fixed air from phlogiston and dephlogisticated air, which I shall mention in this section, is of a much more striking nature than any that I have yet recited. Having made what I call *charcoal of copper*, by making vapour of spirit of wine pass over copper when it was red hot, I took a piece of it, and, with no very particular view, heated it in different kinds of air. Among others, I did this in common air, and not observing any increase or decrease of the quantity of air, concluded, but perhaps too hastily, that no change was made in it. For when I repeated

repeated the experiment in dephlogisticated air, the charcoal burned very intensely; and when a part of it was consumed (which, like common charcoal in the same process, was done without leaving any sensible residuum) I found that no heat which I could apply afterwards had any farther effect on what was left of the charcoal. Concluding, therefore, that some change must be made in the quality of the air, I examined it, and found about nine-tenths of it to be the purest fixed air, and the residuum was such as would have been made by separating the absolutely pure part of the dephlogisticated air, and leaving all the impurities in what remained.

Having ascertained this fact, I repeated the experiment, weighing the piece of charcoal very carefully before and after the process, and then found that, by the loss of one grain of the charcoal, I reduced four ounce measures of dephlogisticated air till one-ninth only remained unabsobered by water; and again, with the loss of one grain and

and an half of the charcoal, I reduced fix and an half measures of dephlogisticated air till five and a half ounce measures were pure fixed air.

In this process there was a diminution of the bulk of the air after the experiment, as might be expected from the change of the air into one of a heavier kind by means of a substance, or principle, that could not add much to the weight of it; but I did not accurately measure this. In one of the experiments 4.3 ounce measure of dephlogisticated air were diminished, I observed, about one-thirtieth part of the whole. But being in a pretty wide vessel, such a measure cannot be accurate enough for computation. In this case, when the fixed air was separated by water, there was a residuum of 0.75 of a measure of the standard of 1.0, whereas the dephlogisticated air before the experiment had been of the standard of 0.2.

That dephlogisticated air actually enters into the composition of this fixed air in this experiment, is evident from the weight
of

fin v. in 9
 of the fixed air, which far exceeds that of the charcoal, which is dispersed in the process. For in this last experiment the weight of the fixed air produced was 4.95 grains. Consequently, supposing the charcoal to be wholly phlogiston, as it is very nearly so, fixed air may be said to consist of 3.45 parts of dephlogisticated air, and 1.5 phlogiston. So that the dephlogisticated air is more than three times the proportion of the phlogiston in it.

I must not conclude this section without observing that, in one experiment, I never failed to produce fixed air, though it is not easy to see how *one* of its supposed elements, viz. dephlogisticated air, could enter into it. This is by heating iron in vitriolic acid air. I repeated the experiment many times, and always had this very remarkable result.

In one of the experiments, four ounce measures of the vitriolic acid air were reduced to 0.65 of an ounce measure; and of this three and one-half of the whole was fixed air, absorbed by lime-water, and the remainder

remainder was slightly inflammable. In another experiment I could not perceive any thing inflammable in the residuum. It appeared to be only phlogisticated air. But these residuums are always small, so that it is not easy to distinguish weakly inflammable air from that which is phlogisticated.

SECTION XV.

Of the Change made in Fixed Air by the electric Spark.

I Have observed in my former publications, that by taking the electric spark in fixed air, a part of it is converted into air that is not absorbed by water. I have since repeated this experiment with more care; and though I have never been able to make the whole of any proportion of fixed air immiscible with water by this means, yet I have always so far changed it,

that the residuum was more considerable than before, but in different proportions.

I took the electric spark about two hours in a small quantity of fixed air confined in a glass tube by mercury. Before the experiment, one-thirtieth of the air was unabsorbed by water, but afterwards one-fourth. The glass tube in which this experiment was made became very black in the inside; and as this change is made in mercury by the addition of phlogiston, it looks as if some of the phlogiston, which had made a part of the fixed air, had, by this process, been separated from it; and leaving a greater proportion of dephlogisticated air in the remainder, would necessarily make it less miscible with water. The blackness on the inside of the tubes, in which the electric spark is taken through vitriolic acid air or common air, I before discovered to be mercury supersaturated with phlogiston.

The next time that I repeated this experiment, I attended to the quality of the residuum before and after the process; and the result was such as seems to confirm the
above-

above-mentioned conjecture. I took the electric spark an hour and ten minutes in little more than half an ounce measure of fixed air, after which one-fifth of the whole was unabsorbed by water, and the standard of the residuum was 0.9. Of the original fixed air about one-thirtieth was unabsorbed by water, and the standard of the residuum was 1.0. In this experiment I also observed that the quantity of the air in which I made the experiment was increased about a twentieth part, which I do not pretend to explain.

Again, I took the electric spark an hour in half an ounce measure of fixed air, after which there remained as much residuum unabsorbed by water as had remained in about five times the quantity of the same fixed air in which no spark had been taken. This residuum was also much purer than that of the original fixed air, the standard of it being 0.8, whereas that of the original fixed air had been, as before, 1.0. I repeated the experiment, and found the residuum still greater, but of the same pure

quality; and in this case I observed a good deal of the black matter adhering to the inside of the tube.

In the following experiment I observed a farther change in this substance. In a small tube, containing about one-fifteenth of an ounce measure of fixed air, I took the electric spark about an hour; after which there was a good deal of the black matter clouding all the inside of the tube; but the lower part of it was covered with something of a yellow colour, like sulphur. In this case the residuum not absorbed by water was between one-fourth and one-fifth of the whole, and less pure than the former residuums. Had not the dephlogisticated air in the fixed air passed into the mercury, tending to make it a precipitate *per se*? Was not this the cause of the residuum being less pure than before? And does not this experiment also prove, that phlogisticated air may be composed of the same materials with fixed air, viz. dephlogisticated air and phlogiston?

Again,

Again, I took the electric spark three hours in a small quantity of fixed air, and observed that it was first increased, and then diminished about one-eighth of the whole; the inside of the tube being very black, and below the mercury very yellow, about the space of a quarter of an inch quite round the tube. But that space, or at least part of it, had been above the mercury at the beginning of the process. There remained one-third of the air unabsorbed by water, and so impure, that the standard of it was 1.8.

To vary the experiment, I took the electric spark in a quantity of fixed air confined by water, impregnated with fixed air. The quantity was much increased by the air extricated from the water, and after the process by far the greater part of it was incapable of being absorbed by lime water. In the course of this experiment, I observed that water impregnated with fixed air is by no means so good a conductor of electricity as water impregnated with any of the mineral acids.

Again I took the electric spark in fixed air, confined by a little common water, and observed that the blackness mentioned above extended more than a quarter of an inch below the surface of the mercury, in the same manner as the yellow colour had done before. In this case also, the residuum was purer than that of the original fixed air.

Again I took the electric spark half an hour in seven tenths of an ounce measure of fixed air, after which one-tenth of it was immiscible with water, and the residuum was evidently better than the natural residuum of the same fixed air. The standard of that had been 1.0, and of the other about 0.85.

I took the electric spark three hours in about three fourths of an ounce measure of fixed air, after which it was increased in bulk one eighteenth. Water being admitted to it, there remained one-sixth unabsorbed. Being examined, the standard was found to be as before, a little better than the residuum of the same fixed air.

Being

Being desirous of ascertaining whether this change in the constitution of the fixed air was owing to the *light*, or the *heat* produced by the electric spark, or to something peculiar to electricity. I first threw a strong light by means of a burning lens, on some pounded glass, confined in fixed air, for some hours. But though the residuum was by this means a little increased, yet being of the same quality with the common air, I suspected that it was the air which was necessarily introduced through the quicksilver along with the pounded glass. There was no change in the dimensions of the air after the experiment.

I repeated the process with fine glass-house sand, which had been previously exposed to a strong heat. But though the residuum was increased, the experiment was not, upon the whole, more satisfactory than the former. I also heated bits of crucibles in the same manner, and found the residuum larger than before, in the proportion of 10 to 6.6; but the quality of it was worse.

To what this should be owing, I cannot tell.

I once more repeated the experiment with bits of crucibles, and the result was certainly favourable to the hypothesis of a real change being made in the quality of the air by *heat*, but I do not pretend to say that it was decisively so. After the process with fifty-six measures of the air there was a residuum of three measures; whereas before the experiment, the same quantity of the fixed air had left a residuum of only two measures. And that the additional measure was not the common air, introduced into the vessel by adhering to the bits of crucibles, was evident from the quality of the residuum, which was the very same, viz. of the standard of 1.1. I also assured myself that there was no fallacy of this kind in the experiment, by introducing the very same bits of crucibles into another equal quantity of fixed air. For I did not find that any sensible quantity of common air had been carried into the vessel along with them.

However,

However, by heating *iron* in fixed air, there can be no doubt but that a sensible quantity of it is converted into phlogisticated air; which agrees with the experiments that I formerly made, by putting pots of iron filings and brimstone into fixed air. The experiments that I made of this kind were the following, in which it will be observed, that, though in some of them, there was an increase of the quantity of air after the process, yet that it was by no means equal to the quantity that remained unabsorbed by the water; and therefore, there must have been a farther addition made of this kind of air in the process.

After heating turnings of malleable iron in a quantity of fixed air for some time, I examined a part of it, and found that about one-tenth of the whole was immiscible with water. Having resumed the process with the remainder, I found a residuum of one-fourth of the whole. There seemed to be a small addition to the quantity of air after the first part of the process, but I could not perceive that there was any
after

after the second. I resumed the process a third time, but did not find that I had made more than one fourth of the whole immiscible with water. At another time I heated the same kind of iron in fixed air, till of $3\frac{3}{4}$ ounce measures of air there was a residuum of 0.8 of a measure, which was slightly inflammable, burning with a blue flame; and in this case there was no sensible addition to the quantity of air at all. Lastly, I heated iron in three ounce measures of fixed air till there was an addition of 0.4 of a measure to the quantity of it; but there was a residuum of one and a half of a measure not absorbed by water, which burned with a slightly explosive blue flame.

SECTION

SECTION XVI.

Of the Quantity of Fixed Air expelled from various Substances.

AS it is sometimes useful to know what quantity of fixed air may be expelled from certain substances in particular states, and also what proportion of other kinds of air may be mixed with it, I shall mention such processes of this kind as I have notes of. They are similar to some of those in which air was extracted from mineral substances.

The purest calcareous earth is *chalk*, and the most perfect chalk is that which is called *whiting*, which is therefore useful in many experiments, so that it is worth while to know what air it contains. From seven ounces of this substance, I got, in an earthen retort, 630 ounce measures of air, by which it was reduced to four ounces.

Every

Every portion of the air contained about one-third that was not fixed air, the standard of which was 1.36, 1.38. Again, from six ounces of whiting, I got 440 ounce measures of air, about half of which was fixed air, and the remainder of the standard of 1.4. The whiting was reduced to three ounces and 312 grains.

In order to try whether any peculiar kind of air might be procured from whiting saturated with acids, I moistened some, which had been well calcined, with water impregnated with vitriolic acid air; and then by heat expelled from it 90 ounce measures of air, the former part of which was more than three-fourths fixed air, and the residuum of the standard of 1.5. The last portion had less fixed air in it, and the standard of the residuum was 1.44. The substance was rendered black and hard, but in spirit of salt it became white and soft.

When *quick lime* is suffered to fall in the open air, it first attracts moisture, and then that moisture gives place to fixed air. From three ounces and a quarter of this
fallen

fallen lime I got 375 ounce measures of air, of which about one-fifth was fixed air, and the standard of the residuum was 1.4.

From four ounces of *white lead* I expelled 240 ounce measures of air, before the retort was dissolved by it. Of the first produce there remained one-third, not fixed air, of the standard of 1.36; and towards the last, the residuum was of the standard of 1.28, when with the common air it was 1.23.

From two ounces and three quarters of *wood ashes* I got, in a very strong heat, 430 ounce measures of air, of the first portion of which one-tenth, of the second one-third, and of the third one-half was fixed air. The residuum of the second portion was of the standard of 1.6, and that of the third 1.7. It extinguished a candle; so that the air came properly from the *ashes*, and not from any remaining particles of the charcoal mixed with them. After the process, the ashes weighed 839 grains. Being exposed to the open air one day, they weighed 842 grains, and, perhaps with more heat than before, yielded 30 ounce measures

measures of air, of which about an eight was fixed air, and the standard of the residuum was 1.38, and 1.41. A candle burned in it; so that it is evident some of the dephlogisticated part of the atmosphere had been imbibed by these ashes. They then weighed 789 grains and a half.

From three ounces of *pit-coal ashes*, I got air, the standard of which was 1.7, and extinguished a candle. I took no note of the quantity of fixed air, and through an accident in the process most of the air escaped.

HOMBERG'S *pyrophorus* is a substance liable to be spoiled by exposure to the air. I prepared some of it, with a view to observe what it imbibed from the air, and with it I made the following experiments. It was composed of two parts burned alum, one of salt of tartar, and one of charcoal. About two ounce measures of this pyrophorus I suffered to burn in the open air; and then in an earthen retort, I extracted from it 144 ounce measures of air, of which one half at the first was fixed air, but at the last very little. The residuum
of

of the first portion extinguished a candle, but that of the last burned with a lambent blue flame. When examined with nitrous air, both the residuums were of the standard of about 1.8.

The pyrophorus was then kept two days in the retort, with its mouth in mercury, and then being taken out, it presently grew hot, and burned as well as ever. Immediately before the burning it weighed 428 grains, immediately after it 449 grains. Having been spread thin, and exposed to the atmosphere, the next morning it weighed 828 grains; but when well dried, it weighed only 486 grains. It was then subjected to a greater heat than before, and it yielded 110 ounce measures of air, the first portions of which were half fixed air; but the last contained very little, and burned with a lambent blue flame. The substance then weighed 396 grains.

Afterwards I took a quantity of pyrophorus, which would not take fire in the open air, and heating it in an earthen retort, found five-sevenths of the first part of the produce

produce fixed air; but this proportion gradually diminished, till at last nine tenths of the whole was inflammable air, burning with a lambent blue flame. This inflammable air being decomposed with an equal quantity of dephlogisticated air, yielded 0.86 of a measure of fixed air.

Another quantity of pyrophorus, which had burned very well, and which, during the burning, and in two days exposure to the atmosphere afterwards, had acquired 132 grains in weight, being again exposed to heat, in an earthen retort, gave an hundred and eighty ounce measures of air, of which the first portion was three-sevenths of fixed air, and the rest phlogisticated. But afterwards one-half only was fixed air, and the rest inflammable, burning with a lambent flame, and at last it was wholly inflammable. When this pyrophorus was cool, it took fire again by exposure to the open air, but not without the assistance of some external heat. It had been red hot through its whole mass at the first burning of it, and had continued so a considerable

time, after which the surface of it was slightly covered with white ashes; but all the inside was as black as ever it had been.

From four ounces of dry *ox blood* I got 1200 ounce measures of air, and I conjectured that not less than 200 ounce measures escaped. It contained no fixed air. The first portion of it burned with a large lambent white flame, the middle portion fainter, and the last was hardly inflammable at all, but had a slight blue flame. What remained of the blood weighed 255 grains, and was a very good conductor of electricity, which is not usually the case with the charcoal of animal substances.

SECTION XVII.

Experiments relating to Dephlogisticated Air.

SINCE the discovery of dephlogisticated air, I have been led to form different opinions concerning the constitution of it. At first I thought it consisted of earth and spirit of nitre, because, in every process from which I then procured it, I used some kind of earth, but especially the metallic ones, moistened with spirit of nitre, and there was a loss of the earth, as well as of the acid, in every process. Afterwards I was convinced, that the earth was only dispersed in the process, and therefore concluded, that this air was merely *nitrous acid* in another form, with the addition of the principle of *heat*, or whatever else might be necessary to give it the form of air. Since that time Mr. WATT

has

has led me to think, that there is no acid whatever in dephlogisticated air, and that it consists of nothing more than one of the constituent parts water, combined with the element of *heat*; the other constituent part of water being inflammable air. In this opinion I for the present acquiesce; and in this section I shall recite some experiments which favour this hypothesis, as well as others of a miscellaneous nature, relating to this species of air.

The first observation that I shall make respects the *production* of this air. At first I procured it from precipitate *per se*, which is an expensive article in chymistry, and then from red precipitate, which, when carefully prepared, so as to expel from it all the acid, is the very same thing. I have frequently observed that an ounce of this substance will yield 60 ounce measures of the purest dephlogisticated air; and it is something remarkable, that an ounce of *turbith mineral*, which consists of mercury and the vitriolic acid, yields about the same quantity of air, and equally pure.

For a considerable time I was not able to procure dephlogisticated air by means of any other acid than the nitrous. But afterwards I got it by means of the vitriolic, as in the case of turbith mineral. Other persons have procured it by means of the marine acid, as Mr. LANDRIANI, who first succeeded in this process, and then Mr. BERTHOLET. I have now procured this air by means of the *acetous acid*, and though the fact is but of little consequence, and the experiment was made with another view, it may not be amiss to recite the particulars of it.

Having mixed half an ounce of *radical vinegar* with two ounces of calcined whitening, I got from it, by heat, in an earthen retort, 350 ounce measures of air, of which about one-third was fixed air, but more than this in the first portions, and less in the last. The standard of the residuum in the first portions was 1.66, in the second 1.42, and in the third 1.38; which was very near the purity of common air. The whitening then weighed 760 grains. I then
put

put a quarter of an ounce more radical vinegar to it, and without taking any account of the quality of the air, only observed that I got 120 ounce measures from it, and that the residuum weighed 730 grains. After this I put to it a quarter of an ounce more of the same acid, and got from it 140 ounce measures of air, of which the last portion had in it no fixed air, and was considerably better than common air; but of the exact standard of it I find no note. The whitening then weighed 489 grains. The air had always been very turbid, which accounts for the continual loss of weight in these processes.

The cheapest method of procuring dephlogisticated air in large quantities that we are yet acquainted with, is to extract it from nitre by means of heat; and in general no more has been got from an ounce of nitre than about 100 ounce measures of air. The reason of which has been, that, in order to get it pure, *glass vessels* have been used; and after a part of the acid has been expelled, the remaining alkali has dissolved

the glass, and the process has of course terminated. But by means of Mr. WEDGWOOD's excellent earthen retorts, I have been able to get such quantities of this air from nitre, as to myself, and all my friends, appeared very extraordinary; and has assisted Mr. WATT in proving that there is no acid in this species of air. As only the general results of these experiments have been quoted by Mr. WATT, and others, I shall recite some of the particulars in this place.

The first time that I got more air than usual from nitre was in a coated glass retort, when, from two ounces of it, I got 300 ounce measures of air. But from the same quantity of *purified nitre*, in one of Mr. WEDGWOOD's retorts, I got 787 ounce measures. The air procured in this manner is never of the purest kind; and the standard of this, from the first to the last, with two equal measures of nitrous air, was only 1.25. I conducted this process so that the air was very little turbid, and yet there was something acid discovered by

by the smell in every portion of it. In another process I got 796 ounce measures of air, and the retort was dissolved before the process was completed. But the largest quantity that I ever got from two ounces of nitre was 812 ounce measures, and this was purer than any I had got before in this way, the standard of the greatest part of it being 0.95, though at the very last 1.5. In all cases I have observed, that the purer the air is, the greater is the quantity. For it can only be rendered impure by some phlogistic matter uniting with the purest part of it, and forming, in many cases, as I am inclined to think, phlogisticated air. In this experiment the loss in the nitre was 531 grains. The air would have weighed something more than 487 grains, so that more than one-half of the weight of the nitre was reduced to the form of air, and there remained 34 grains for the water of crystallization, &c.

In these experiments it was a matter of some consequence to determine whether any acid remained in the nitre after all the air

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was come over, and as the vitriolic acid has a stronger affinity with the base of the nitre, there appeared to be no better method of determining this, than to put oil of vitriol to it, and endeavour to make nitrous acid. Accordingly, in the former processes, I had always been able to get nitrous acid, or nitrous air, from what remained of the nitre, after the dephlogisticated air was expelled from it. Thus from what remained of one of the preceding processes, I had got nine ounce measures of nitrous air from half an ounce of the saline residuum. And as it was evident that the nitrous acid in this nitrous air was much less than had been contained in the nitre, I had been used to conclude, that the acid had been converted into dephlogisticated air. But I was led to suspect this conclusion, from observing that the dephlogisticated air had nothing sensibly acid in it, not only while it existed in the form of air, but also when decomposed with inflammable air; and I had always observed, that every vessel of the air, fresh received from the nitre, had a pungent

pungent smell, which must have proceeded from some volatile acid diffused through it, and also the water in which the air was received had acquired a slight acidity, though it was of a very volatile nature.

To these circumstances I gave particular attention in my future experiments. To that which remained of the last mentioned experiment, in which I procured 812 measures of air from two ounces of nitre, I put oil of vitriol and water. But though it was exposed to heat till the water was distilled over, I did not get half an ounce measure of air more than the vessel contained; and this was dephlogisticated. Consequently, all the nitrous acid had been expelled from this nitre.

The colour of the alkaline matter to which the nitre was reduced in these earthen retorts, was a deep green, owing, perhaps, to something of the earth in the retort; for in a porcelain vessel, I, at one time, had it perfectly white.

At Mr. WATT's request, I endeavoured to ascertain the quantity of acid that was expelled

expelled from nitre, in procuring the dephlogisticated air from it. To do this, I put two ounces of purified nitre into a glass retort, and receiving the air in 300 ounce measures of water, only filled each recipient half full, and agitated the air very much in the water, in order to make the water imbibe as much as possible of the acid that it contained. However, notwithstanding this agitation, every vessel of the air retained a strong smell of the acid. The moment the air ceased to come, I filled a large phial with the water, and carried it to Mr. WATT, who carefully examined it; and in a paper which he presented to the Royal Society, and which is published in the Philosophical Transactions, he has given an account of the quantity of acid that it contained, computing from the specimen that I brought him the quantity that was contained in all the 300 ounce measures of water; from which it may be fairly inferred, that there was no occasion to suppose that any of the acid entered into the composition of the air, but that it was all

all either rendered volatile, or retained in the water.

I afterwards examined a portion of this water, by expelling air from it, and did not find it to contain more air than water generally does, but what I did get from it was considerably phlogisticated.

SECTION XVIII.

Of the constituent Principles of Nitrous Air.

THERE is no kind of air, the constituent principles of which seemed to be more clearly ascertained than those of nitrous air, by those philosophers who admitted the doctrine of phlogiston. We took it for granted, that it consisted of nitrous acid and phlogiston. In an early period of my experiments I endeavoured to ascertain the quantity of acid contained in it.

it. Mr. KIRWAN has done the same, and Mr. CAVENDISH also has considered it as containing a very concentrated nitrous acid. I had no doubt on the subject till I read the work of Mr. METHERIE, who asserts that nitrous air contains no proper nitrous acid, but only one of the elements of it, the other being dephlogisticated air, which had before been considered by Mr. LAVOISIER as the principle of all acidity.

Among other observations in support of his assertion Mr. METHERIE has the following. Nitrous air burned together with inflammable air, produces no nitrous acid, p. 145. Though nitrous air is obtained from a solution of mercury in nitrous acid, almost all the acid is found in the solution, p. 146. Nitrous air absorbed by marine acid, does not make aqua regia, p. 153. He is of opinion that a small portion of the nitrous acid being decomposed, furnishes a pure air, so altered that, uniting with inflammable air, it changes it into nitrous air, p. 150.

As to this *hypothesis* of Mr. METHERIE, I cannot say that I well understand it; but, being

being struck with his *observations*, I began to review the experiments which I had formerly made on this kind of air, and could not recollect any of them in which it appeared that real nitrous acid was produced, except when nitrous air was combined with dephlogisticated air, besides the experiment in which it was decomposed by the electric spark, which furnishes a strong objection to this hypothesis. In every other view Mr. METHERIE's theory appeared to me not improbable, and led me to make the following experiments, which though not decisively in favour of it, may deserve consideration. They will at least shew that, if there be all the elements of nitrous acid in nitrous air, they must together with an additional portion of phlogiston exist in the phlogisticated air that is formed in the processes.

When nitrous air is decomposed by iron, or by a mixture of iron and sulphur, I have observed that the water over which the process is made, acquires no acidity by it; but I had supposed that all the acid had been seized by the iron. Having by me a considerable

considerable quantity of this iron which had been reduced to a perfect rust in nitrous air, and which I knew must have imbibed more than its weight of this air, I thought that I might get the acid out of it by distillation, if it contained any. But a quantity of this rust of iron, carefully distilled in an earthen retort, yielded neither nitrous air, nor nitrous acid, at least in any quantity that could favour the common hypothesis. For besides the *air* (of which an account will be given in its proper place) some ounces of this rust yielded only a few drops of liquor, which was not sensibly acid to the taste, and when poured into a little water, tinged blue with the juice of turnsole barely turned it red. But so slight a degree of acidity as this might have come (and I have no doubt but did come) from the union of nitrous air with those bubbles of common air which could not be excluded from the vessels containing the iron, in the many times in which I changed the air, when I supplied them from time to time with fresh nitrous air; so that the great mass of nitrous acid which ought

to have been deposited in this iron must have been concealed in the phlogisticated air, which is generally about one fourth of the original quantity of nitrous air. This brown rust (become so by exposure to the air, which it imbibes) became black by the expulsion of its air and water, and exactly resembled that iron ore which is found in the form of a black sand.

I then endeavoured to decompose nitrous air by heating iron in it with a burning lens; and in this process, I succeeded much beyond my expectation. For the air was presently diminished in quantity, while the iron became of a darker colour, was sometimes melted into balls, and gained considerable weight. It had no appearance, however, of containing any nitrous acid, and yet water tinged with the juice of turnsole introduced into the vessel, and agitated in it after the process, never changed its colour.

In the first experiment the original quantity of nitrous air was diminished to about one third, and after this it was increased. But then I found that when I had got to the
extreme

extreme of diminution, there began to be an addition made of inflammable air from the iron; for this was evidently so. It was also observable that there was either some pure air, or dephlogisticated nitrous air, in what remained of this process. For when a measure of this residuum was mixed with an equal measure of common nitrous air, they were reduced to 1.7. I suspect, however, that this was owing to a quantity of dephlogisticated nitrous air being formed by this process, as it is similar to the rusting of iron in nitrous air.

In another process of this kind, I reduced six ounce measures of this air to three, and this was wholly phlogisticated air, extinguishing a candle. I also heated iron in seven ounce measures and a half of nitrous air, till it was reduced to 3.7 measures, when it appeared to be perfectly phlogisticated air. The iron had then gained $2\frac{1}{2}$ grains in weight, and at this time I observed that there was not the least appearance of moisture produced in the course of the experiment.

III. Then

Then recollecting that, in my former experiments, HOMBERG's pyrophorus had taken fire in nitrous air, I made a quantity of this substance, and repeated the experiment with the greatest care. But when nitrous air was decomposed by this means, I saw no reason to think that any nitrous acid had been formed. In this process the nitrous air is readily diminished to about one-half of its bulk, after which nothing nitrous is found in it, but a portion of dephlogisticated nitrous air, as appears by its being much more readily absorbed by agitation in water than nitrous air is. But neither the air, nor any thing that can be expelled from the pyrophorus, discovers any sign of containing nitrous acid.

Having saturated a quantity of pyrophorus with nitrous air in an earthen tube, which by this means became very hot, I admitted water to it; and distilling the pyrophorus with this water, the liquor that came from it would not change the colour of the juice of turnsole; and the air which came afterwards was of the same

kind with that which is always expelled from pyrophorus after it has been burned in the open air, viz. partly fixed air, and partly inflammable.

I shall just mention some farther particulars of another experiment of this kind, though it was made long before I had the views which led to these now mentioned. Having saturated about two ounce measures of HOMBERG's pyrophorus with nitrous air, I observed that it had imbibed about four times its bulk of the air. This pyrophorus was of a kind that did not burn readily in the nitrous air, or I believe in common air, without external warmth. Having withdrawn it from the nitrous air, it took fire again, but with more external heat. Then putting it into an earthen retort, I expelled from it ninety ounce measures of air, a very small proportion of which was fixed air. The former portions of it extinguished a candle, and were of the standard of 1.7, and the latter of 1.55, burning with a lambent blue flame. Three days after this, being taken out of the retort (the mouth of which had

had been all the time immersed in mercury) a small part of it took fire. I observed it then weighed 416 grains, and the next morning it weighed 438 grains.

There is something very remarkable in this substance burning equally well in two such very different kinds of air as nitrous and dephlogisticated. It should seem from this circumstance, that there must be some common principle in the constitution of them both; though from their so readily uniting together, they seem to be things as different from each other as acid and alkali. This well deserves a careful investigation.

In my former experiments I found that the juice of turnsole became red by being impregnated with nitrous air. I found, however, that this must have been owing to some extraneous acid that was mixed with it. For when I took nitrous air that had stood long in water, and transferred it into a vessel of mercury, I impregnated with it water tinged with the juice of turnsole without producing any change in its colour.

I have also observed that alkaline air admitted to nitrous air, made a whitish cloud in it. But this must have been owing to the same cause; the nitrous air in that experiment having been probably fresh made. For in the nitrous air which had been long confined by water, the mixture of alkaline air, did not produce the least cloud. I perceived, however, that after the two kinds of air has been together a day or two, there was a small quantity of a very deeply tinged blue liquor in the space between the mercury and the glass; which shows that there was copper dissolved in the nitrous air, and that it continued there after the extraneous acid was separated from it. On this copper the volatile alkali had seized.

Notwithstanding these results, when the electric spark was taken in the very same nitrous air, with which the preceding experiments were made, the juice of turnsole by which it was confined, presently became red. This circumstance deserves to be examined with care, as it seems to be decisively in favour of the nitrous air containing

containing all the necessary elements of nitrous acid. An account of some experiments of this kind will be found in the next section.

SECTION XIX.

Of Nitrous Air in general.

THE experiments that I have lately made on the subject of nitrous air, relate chiefly to that modification of it which, from its property of admitting a candle to burn in it, I have denominated *dephlogisticated nitrous air*; but before I proceed to these, I shall, in this section, recite a few others of a miscellaneous nature.

Water seems to be a necessary ingredient in nitrous as well as in inflammable air, at least without a quantity of water, nitrous air cannot be formed. For example, cop-

per will be dissolved in strong nitrous acid, but without producing any air, just as inflammable air is not produced unless the vitriolic acid in which the iron is dissolved be much diluted with water.

That nothing is necessary to the formation of nitrous air besides phlogisticated nitrous acid and water, is evident from the production of it by the impregnation of pure water with phlogisticated nitrous vapour, formed by the rapid solution of bismuth; an experiment which I have mentioned before. However, to make it in a more unexceptionable manner, I interposed a glass vessel between that in which the solution was made, and that in which the water to be impregnated with the phlogisticated vapour was contained; that whatever of the solution was distilled over by the heat of the process (which evidently occasioned the dropping of a blue liquor from the end of the tube through which the vapour was delivered) might be prevented from reaching the water. In these circumstances, however,

however, when nothing but the dry phlogisticated vapour could enter the water, it began to sparkle, and yield nitrous air very copiously, as soon as it had acquired a blue tinge from the impregnation. This is a very striking experiment to all persons when they have seen it the first time.

Nitrous air is also produced by pouring a highly coloured, or phlogisticated, nitrous acid into pure water, in which no metal or earthy matter is any way concerned.

I have formerly observed, that a solution of green vitriol readily decomposes nitrous air, and thereby becomes very black, but that if this black solution be exposed to the air, it loses its colour again. Having impregnated a quantity of the solution in this manner, I heated it in a glass phial, and found that it gave out half its bulk of pure nitrous air, but when it had lost its dark colour, it gave no air at all by heat.

I have likewise observed how readily nitrous air is diminished by taking the electric park in it. This experiment I have fre-

quently repeated, in order to ascertain more particularly the quantity and quality of the residuum. In one experiment I reduced half an ounce measure of nitrous air, in less than half an hour, to one quarter of its bulk; but then one-fourth of it was still nitrous, and the rest phlogisticated air. Receiving it in lime water, there seemed to be a slight precipitation; but this circumstance will perhaps be explained in the following experiment.

I took the electric spark in a quantity of nitrous air till there remained no more than one-third of it, and this was completely phlogisticated, not affecting common air at all, and extinguishing a candle. A white matter was formed with the mercury over which the spark was taken, which made the water admitted to it extremely turbid. At that time I supposed that this white substance was mercurial nitre, consisting of nitrous acid from the decomposed nitrous air and mercury. I shall now examine it more particularly. Why there should be more than one-third of this quantity of
nitrous

nitrous air left pure phlogisticated air, and not one-fourth, as in the former experiment, I cannot tell.

In another process I took the electric spark in a quantity of nitrous air, till it could be no more diminished; when it was in proportion to its former bulk, as 10½ to 24. Letting it stand all night upon the mercury, it was increased to the proportion of 11¼ to 24, seemingly by the acid uniting to the mercury, and generating more nitrous air, since it had that smell. No water appeared after the process, and the water admitted to it acquired no acid taste, but an astringent one, like water impregnated with nitrous air. There was a white powder formed, as in the former experiments.

Thinking to make water imbibe the acid from the nitrous air, decomposed in this process, I took the electric spark in it with a small quantity of water over the mercury. But even this water did not acquire any acid taste, but only an astringent one.

It remains to be ascertained on what principles the nitrous air is decomposed in this

process. That it is not by means of the *heat* communicated to it, may, I think, be inferred from an experiment, in which I made a quantity of nitrous air pass through a red-hot tobacco pipe, without producing any change in it. Neither does it seem to be the *light* of the electric spark, since I have heated pieces of crucibles in nitrous air by the sun beams, without making any change in it.

Upon the whole, it may be most probable that this change depends upon *phlogiston*, as it is similar to that which is made in nitrous air by the contact of iron, and especially by heating iron in this air. There is this difference, however, in the two processes; that in that with electricity the residuum is wholly phlogisticated air, and the intermediate state of dephlogisticated nitrous air is not found. At least I have not observed it.

SECTION XX.

Experiments relating to Dephlogisticated Nitrous Air.

BEFORE I recite the experiments which I have made on this kind of air, I shall mention some observations on the process by which it is procured. And the first thing that I shall observe is, that *water* is absolutely necessary to this decomposition of nitrous air, when it is effected by iron. I had decomposed it before, either by previously filling the vessels that were to contain the nitrous air with *water*, or with *mercury*, though it had always required much longer time when mercury was used. The reason of this difference I did not then know; but I now perceive that it was owing to a small quantity of *moisture* in the vessel containing the mercury.

Suspecting

Suspecting the influence of *water* in this process, I procured a number of very clean small needles, and having made a phial, and also a proper quantity of mercury, perfectly clean and dry, I put the needles into the phial; and filling it up with mercury, introduced the nitrous air. This phial continued in the same state six or eight months, without the least sensible change in the space occupied by the air. I therefore concluded that no change had taken place in it, and therefore introduced a few drops of water; after which I perceived, in a day or two, that the air was diminished, and the diminution proceeded till about one-third of the air had disappeared. In this state the phial continued several months more; when, observing no farther change, I examined it, and found that a candle would not burn in it, which I had expected it would have done; as I imagined there was not water sufficient to imbibe that part of the air which had enabled a candle to burn in it; and how this part of the air should disappear, or be converted into phlogisticated

cated air, without water, I could not tell. In another experiment the result was seemingly different.

On the 26th of May, 1782, I examined a quantity of nitrous air, which had been confined by mercury with iron shavings from the 27th of August preceding, when I found one-half of it absorbed, and a candle burned in the remainder better than in common air, though a mouse died in it; and yet this air had remained several months in the same state, with respect to quantity, in which it was when I examined it; so that I concluded it would never have extinguished a candle, in consequence of any longer continuance in that state. In this case, however, there was no sensible quantity of water at all; whereas in the other there were a few drops. Water, therefore, may contribute to this change in the air, independently of its *absorbing* any part of it. When I agitated this air in water, something more than one-half was absorbed, and the remainder extinguished a candle.

But,

But, perhaps, the more probable inference from this experiment may be that, in time, all the dephlogisticated nitrous air is imbibed by the iron, so that nothing besides the phlogisticated air will be left in the vessel; though there was no appearance of this being the case in the experiment with the needles; as the air did not continue to diminish longer than usual, and there remained two-thirds of the original quantity of phlogisticated air. This, therefore, rather looks like a change of the dephlogisticated nitrous air into phlogisticated air, by the addition of phlogiston from the iron.

Another experiment I made with a view to ascertain the degree of diminution at which the nitrous air begins to be decomposed. On the 14th of May, I put twenty-four ounce measures of nitrous air into a quart bottle, previously filled with pieces of iron, that had before been rusted in this process. On the 18th there were only seventeen ounce measures of air, and it was not at all changed; on the 21st there were thirteen ounce measures, and on the 31st
four

Dephlogistified nitrous Air. . 319

four ounce measures, of which half was then absorbed by water, and the remainder was phlogistified air. This was the greatest diminution that I remember to have observed in this process. But having neglected it from the 21st to the 31st, I did not find the time at which the decomposition first took place; but in general I believe it is when about one-fourth of the quantity is remaining, and that the change takes place pretty suddenly, for I do not remember ever to have found it in a middle state.

Having a quantity of iron that was reduced to a perfect *rust* in these processes, I had the curiosity to heat it in an earthen crucible, in order to examine the *air* that might be expelled from it, and the result was as follows. From ten ounces of this rust, I got about 180 ounce measures of air, mixed at first with much steam. The first sixty ounce measures were chiefly fixed air, with a phlogistified residuum, of the standard of 1.7. The remainder came much more slowly, requiring about four hours continuance of heat. This contained no
mixture

mixture of fixed air, and was so pure, that a candle burned in it pretty well, being of the standard of 1.32. The matter was reduced to one mass, like iron, strongly attached by the magnet. But, indeed, the rust itself had been so before the process.

It seems probable, that the dephlogisticated nitrous vapour is contained in this rust, and that with the addition of the principle of heat it becomes proper dephlogisticated air.

In order to compare this kind of rust of iron with that which is formed by exposure to the atmosphere, I took four ounces of the latter; and, by the same treatment, got from it 180 ounce measures of air, about one-half of which was fixed air, with a residuum of the standard of 1.7; but the bulk of it was inflammable, burning with a lambent blue flame.

Dephlogisticated nitrous air is the term by which I first distinguished this species of air, because it admitted a candle to burn in it (as I had called that air *phlogisticated* in which a candle would not burn, and that
dephlogisticated

dephlogisticated in which it burns better than in common air, which is also the case with that species of air of which I am now treating) and I now think I see more reason to approve of the appellation. Though this air agrees with dephlogisticated air in admitting a candle to burn in it, and sometimes with peculiar brilliancy, it is as fatal to animals as the most perfectly phlogisticated air. As an attention to the other different properties of these two kinds of air, which agree so remarkably in one circumstance, may be a means of throwing some farther light on the nature of *combustion* and *respiration*, I shall proceed to recite the facts which I have lately observed relating to it.

I have formerly produced this dephlogisticated nitrous air by the contact of nitrous air with iron, and I have not yet completed my observations on this particular process, so as to have ascertained the quantity of nitrous air that a given quantity of iron can decompose, the quantity of iron that a given quantity of nitrous air can destroy, &c. As yet I have seen no iron so

far affected by this process, though it is in a state that easily crumbles to powder, as not to be able to decompose more nitrous air, and almost as readily as fresh iron.

I shall just mention one experiment in which this rusted iron, and fresh iron, were made use of. The nitrous air had been put to each of them on the 27th of August, and they were both examined on the 18th of October; when, in both cases, the air was found to be diminished to about one third, and a candle burned with equal brilliancy in both. Water then absorbed about one half of each of them, and the remainder extinguished a candle. Neither of them had in the smallest degree the property of fresh nitrous air.

This may serve as a recapitulation of my former observations on this subject, and at the same time serve to shew the proper generation of phlogisticated air from nitrous air and phlogiston, agreeably to that most valuable discovery of Mr. CAVENDISH, who has actually produced nitrous acid from phlogisticated air. Here we see that nearly

one fourth part of any quantity of nitrous air may be converted into phlogisticated air; and as the nitrous acid vapour unites with the iron, it is probable that all the phlogiston in the nitrous air, and perhaps also a part of that which belonged to the iron itself, may be employed in the formation of phlogisticated air. For since some pure air is expelled by heat from the iron that has been used in this process, it is possible that it had been deprived of some of its phlogiston. But more data may be wanted to determine, from this process, the proportion of phlogiston in phlogisticated air.

This process for procuring dephlogisticated nitrous air requiring much time, I have of late had recourse to another, which has been to dissolve turnings of iron in a dilute solution of copper in the nitrous acid (the same that remains from making nitrous air) mixing with it again about an equal quantity of water. Without this precaution, though the iron will at first be acted upon very slowly, yet the mixture will at length grow so hot, as actually to boil, and

the process will be exceedingly troublesome. It will be necessary, however, previously to heat the solution of copper, in order to expel from it all superfluous nitrous acid, though the keeping in it pieces of copper might answer the same purpose. Without one or other of these precautions, part of the air procured by dissolving iron in it will be proper nitrous air; but with this precaution the whole of it will be such as nitrous air is reduced to by exposing common nitrous air to iron, that is, one third of it will generally be absorbed by water, and the remainder will be proper phlogisticated air. And having this mixture of dephlogisticated nitrous air and phlogisticated air, the former will be procured pure by agitating the mixture in water, which will imbibe it, and leave the phlogisticated part unabsorbed.

A given quantity of water, I find, will take up about one half of its bulk of this dephlogisticated nitrous air; and it does it almost as readily as water imbibes fixed air. The water thus saturated with the air must be

be heated, and then the dephlogisticated nitrous air will be expelled pure, and may be received in vessels containing mercury. I observed, however, that as this kind of air much resembles fixed air, in its properties of being imbibed by water, and being expelled from it again by heat, it does so also in this farther property, that all the air which has been actually incorporated with the water will not be imbibed by water again. But the proportion of this part is much greater in this kind of air than in fixed air. For in this case I have found it to be as 1 to 10; whereas in fixed air it is as about 1 to 30, or 40. This residuum being examined, was found to be air partially phlogisticated, the standard of it being about 1.4, so that a candle will just burn in it; but the quality of this residuum is not always the same. The residuum of fixed air is generally much purer, viz. of the standard of 1.0.

I observed, that when water fully impregnated with this dephlogisticated nitrous air is exposed to the atmosphere, it very soon

parts with it, much sooner than water impregnated with fixed air.

I mixed dephlogisticated nitrous air with all the other kinds of air that I have made, but it did not unite with any of them; and no sensible change was made in it by any of them; or by any of them in it; except that dephlogisticated air was injured, being reduced from the standard of 0.3 to 0.8. But this was, no doubt, by the addition of that part of this air which is not miscible with water, and is considerably phlogisticated.

In this pure kind of dephlogisticated nitrous air a candle burned better than in common air, but not so well as I have frequently known it to do, as it is originally produced mixed with phlogisticated air; which seems to shew that some change is made in it by being incorporated with water. When it was mixed with inflammable air, they were exploded together with violence, very much like a mixture of inflammable and dephlogisticated air; but a mouse died in it as quickly as in fixed air.

It

It was natural to suspect, that there might be something *acid* in this air. But though it was readily absorbed by water tinged blue with the juice of turnsole, it made no change in its colour. Neither was there any thing alkaline in it. For when the slightest tinge of red was given to this liquor by an acid, the blue colour was never in the least degree restored by its impregnation with this kind of air.

The same thing may indeed be inferred from its not incorporating with either acid or alkaline air. In the experiment with the latter it appeared that this dephlogificated nitrous air is lighter than common air. For mixing it with alkaline air, and then putting acid air to it, the white cloud formed by their union was almost wholly in the lower part of the vessel. But it is not so much lighter as inflammable air is found to be by the same experiment. For in this case the white cloud never reaches the top of the vessel; where the inflammable air appears to

remain, intirely unmixed with the alkaline air.

Thinking that, though dephlogistified air ready formed might not affect, or be affected by dephlogistified nitrous air, it possibly might in its first formation; to try this, I revived mercury from red precipitate in this air. But the operation proceeded just as it would have done in vacuo, the mercury being revived very readily, and dephlogistified air produced, without affecting, or being affected by, the dephlogistified nitrous air.

No less remarkable was the result of heating malleable iron in this air. For the bulk of the air was increased, and yet afterwards no part of it was imbibed by water. It was all phlogistified air, without any mixture of fixed air. It seems, therefore, as if this kind of air wanted nothing but phlogiston to make it phlogistified air, and nothing but the principle of *heat*, to make it dephlogistified air.

This

This experiment happening to draw my particular attention, I repeated it very often, and always had the same result, though with variations, depending on the quantity of the air, and the heat I was able to apply. If this was sufficient to melt the bits of iron, the effect was produced very quickly. As the particulars may be of use to future experimenters, I shall recite some of them.

Of three ounce measures and a half of dephlogisticated nitrous air, two ounce measures remained unabsorbed by water after heating malleable iron in it. During the process, the vessel had been full of whitish fumes. What remained of the air was of the standard of 1.7, and extinguished a candle.

Throwing the focus of the lens about five minutes upon some filings of iron in this kind of air, the quantity was increased one tenth. Afterwards water absorbed about one sixth of it, and the remainder extinguished a candle, being of the standard of 1.8.

I repeated

I repeated the experiment with cast iron turnings, and had the same result. About one fifth was added to the quantity, water absorbed about the same proportion of it, and the remainder was of the standard of 1.85. Some little common air may be supposed to have been introduced into the vessel along with the turnings or shavings of iron, but this could not be much; and in all these experiments the addition produced in the process was a very small part of what remained unabsorbed by water; so that there can be no doubt but that this operation produces a real change in the quality of the air, rendering it almost wholly phlogisticated.

In order to produce this effect, the heat must be very considerable, or the pieces of iron very small, so that they must at least be red hot. For when I used whole cast iron nails, on which I suppose the heat of my lens could not make sufficient impression, the effect of applying it an hour and a half, and with a very good sun, was very inconsiderable; the proportion of that part of the air which

was

was immiscible with water being only increased in the proportion of 50 to 27.

But the circumstance to which I happened to give the greatest attention with respect to this kind of air, in the course of the last summer, was the change that is made in it by *heat*, or *light*. For such must be the agents, when bits of clean crucibles, or retorts, are heated by a burning lens in any kind of air. The consequence of this process always was both an increase of that part of the air which will not unite with water, and also rendering it purer than before, approaching nearer to the state of atmospherical air; exactly like the result of taking the electric spark in fixed air. I frequently stopped in the middle of this process, and always found these effects to be more considerable the longer I continued it. I shall give the particulars of those experiments only, on which I place the greatest dependence.

Having impregnated a quantity of boiled water with dephlogisticated nitrous air, I found that the air expelled from it was

again

again absorbed by water, in the proportion of 0.7 to 10.1, and the standard of this residuum was 1.7. I then heated in it pieces of crucibles which had been just before made red hot, introducing them into the air by means of a forceps; and after heating them an hour and a half, in a very hot sun, found that a portion of the air was then unabsorbed by water in the proportion of 4 to 7, and the standard of the residuum was 1.55. I resumed the experiment with what remained of the air, and continued it half an hour longer, when the proportion of the part that was not absorbed by water, was to the other as 26 to 40, and the standard of this residuum was 1.45. No moisture ever appeared in the inside of the vessel, and there was no sensible change in the quantity of the air.

In a portion of the same dephlogisticated nitrous air that was used in the former experiment, I heated bits of crucibles, which, after being dipped in water, had only been dried, though they had been made very hot in the sun. In the course of this process, much

much water appeared in the vessel, expelled no doubt from the earthen ware; and after the process had been continued an hour and a half, the part of the air that was unabsorbed by water, was to the rest as 4.5 to 7, and the standard of it was 1.45.

Examining a quantity of dephlogisticated nitrous air, in a portion of which I was about to heat bits of dry crucibles over mercury, I found that by agitation in water 80.5 measures of it was reduced to 3.5 measures, and the standard of this residuum was 1.38. But of that portion of the same air in which bits of crucibles had been heated about an hour in all, 63 measures were only reduced to 2.1, and the standard of this residuum was 1.38. On this occasion I observed that the inside of the glass vessel was pretty thickly coated with a black matter. Indeed, in the former experiments, it had always acquired a dark incrustation; but I did not know whether it might not have come from the earthen ware. I think it highly probable, that it was mercury super-phlogisticated, as every other black incrustation in processes in which mercury

mercury was used has been. It is probable, however, that some phlogisticated matter passed from the air to the mercury, which would of course render the residuum more phlogisticated, though I do not perceive how it should follow that it should thereby render it less miscible with water. But whatever proportion of phlogiston be necessary to make any kind of air miscible with water, any *change* in that proportion may make it less miscible with it.

In these respects this change in the constitution of dephlogisticated nitrous air, very much resembles that which is made in fixed air, both by the electric spark, and also by heating bits of crucibles in it; an account of which has been given before.

I endeavoured to ascertain whether this change in the constitution of dephlogisticated nitrous air was produced by mere *heat*, without light, by heating the air in earthen tubes or retorts, when their mouths were immersed in mercury; but, to my great disappointment, I found that all kinds of air heated in this manner, came out com-

mon

mon air, or rather air a little phlogisticated. In fact, the earthen vessels, in that degree of heat to which I exposed them, must have been rendered pervious to air, though they were glazed inside and outside, and were perfectly air tight both before and after the experiment.

But the most effectual change that I was able to make in this kind of air, was bringing it to a state in which I doubt not it was respirable, by means of the electric spark. Taking this spark, or making small explosions, in $\frac{1}{4}$ of an ounce measure of dephlogisticated nitrous air, it was rendered wholly immiscible with water, and of the standard of 1.45.

From this experiment it should seem, that there is in this species of air something that is capable of being converted into pure air. This indeed is sufficiently indicated both by a candle burning perfectly well in it, and also by pyrophorus spontaneously kindling in the nitrous air from which it is formed. A judicious prosecution of these experiments may therefore throw light upon

upon the nature of both the kinds of air, and of combustion also. This kind of air, however, though a candle burns so well in it, will not kindle pyrophorus. I put a small cupful of the pyrophorus into a quantity of this kind of air, and let it remain there a considerable time, without perceiving any sensible effect; but when I took it out, and put it into a vessel containing nitrous air, it took fire immediately.

In the course of these experiments with the sun, I observed a remarkable source of fallacy, with respect to the increase of the quantity of air confined by mercury, when there is so much moisture in the inside as to be subject to sudden dilations and compressions. For a considerable quantity of common air would get into the inside of the vessel, when there was the depth of an inch of mercury on the outside of it, and of two or three inches within. In these circumstances I have seen more than an ounce measure of the external air gain admission in less than one minute. This must have been occasioned by the mercury never being in perfect

fect contact with glass; so that when the mercury was in a state of undulation, the air that was confined between it and the glass was continually protruded, and more air from the atmosphere was forced into its place, by the same pressure which supported the column of mercury within the glass. This effect I prevented by having a quantity of water upon the mercury, on the outside of the vessel. For this would be in perfect contact with the glass; and in this case I never found either air or water to get into the vessel to disturb my experiment.

VOL. III.

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SEC.

SECTION XXI.

Experiments with Electricity.

THE application of electricity to chemistry is as yet in its infancy. I have made some use of it in the doctrine of air, and Mr. CAVENDISH has lately made a most capital one. This success should encourage us to apply to it with more assiduity.

One of my first experiments on the subject of air was that by which I procured inflammable air by taking the electric spark in different kinds of oil. In the *acids* no spark can be taken, on account of their being such excellent conductors of electricity. However, a small bubble of air may be left confined by any acid, and in this a spark may be taken, which will affect the contiguous fluid. This I have done, and some of the results are sufficiently remarkable; but similar to the effects of transmitting

ting the liquors in vapour, through a red hot earthen tube, in which case the nitrous and vitriolic acids yield dephlogisticated air.

In order to take the electric spark in *nitrous acid*, I admitted a small bubble of common air into a glass syphon, previously filled with that acid, placing each leg of the syphon in a different vessel, containing some of the same acid, and I transmitted the spark, or shock, through a gold wire, on which the acid has no action. In these circumstances every spark made a considerable addition to the quantity of air, and it appeared to be the purest dephlogisticated air.

With the *vitriolic acid* I had a similar result, but much more time was required to produce the effect. After taking the spark two hours in a bubble of air confined by this acid, in a syphon, the greatest part of which was filled with mercury, and the legs of it standing in basons of mercury, the bubble was not more than doubled in quantity; but the addition that was made to it appeared to be dephlogisticated air, the

standard of it being, with equal quantities of nitrous air, 1.0. It may be worth noticing, that though the oil of vitriol, used in this experiment, was of a light purple colour (which might be the reason why the the air procured from it was not both more in quantity, and better in quality) the part next to the air became perfectly colourless for the space of about a quarter of an inch.

When I repeated this experiment with common *spirit of salt*, the bubble of air was diminished one fourth; and though the operation was continued a long time, it never increased afterwards, and was simply phlogisticated air. When I used dephlogisticated marine acid, the bubble of air was diminished near one-half, but what remained was still phlogisticated air.

White + O?

With *phosphoric acid*, the air was first diminished one-quarter, and then increased. This air I could perceive not to be affected by nitrous air; and by the redness of the electric spark in it, I have no doubt of its being inflammable air. But the quantity was too small to ascertain it in any other manner.

manner. This acid is always said to contain phlogiston, and by this means it seems to be converted into inflammable air.

I had a similar result when I took the electric spark in air confined by *phlogisticated alkali*. It was first diminished one-fourth, and then increased to its original bulk; but the increase was very slow. When I examined the air, I could only observe that it was not affected by nitrous air. The addition must, I think, have been inflammable air, though the quantity was so small, that the spark had no sensible redness in it.

I shall conclude the account of these experiments with mentioning one of small consequence, relating to the conducting power of substances. Having boiled some linseed oil, I put into it a quantity of pounded glass. When it was cold it was a solid black mass, and conducted electricity perfectly well through its whole substance, and was not, as I had expected, that kind of substance which conducts electricity in a *visible manner*, such as I have described in

my observations on charcoal, and which I had often found to incrust pieces of glass that had been made red hot in vegetable oils. I then melted the whole mass; but though it remained black throughout, and had been covered from all access of the external air, it had no more conducting power than any other kind of glass.

SECTION XXII.

Of the Influence of Light on Vapour of Spirit of Nitre.

LIGHT, besides serving the important purpose of *vision*, is likewise a chymical principle, the effects of which are as yet but little known; though we have seen sufficient reason to conclude, that it is a very important agent in the system of nature.

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Any new facts, therefore, relating to so curious a subject, must be acceptable to the natural philosopher.

After the experiments which I had before made on the colouring of spirit of nitre, by which it appeared to depend upon phlogiston, it was suggested to me by a philosophical friend, that the *air* incumbent upon the acid might possibly affect its colour, and I was desired to attend to that circumstance. Accordingly I did so, and found that air *as such* had no influence in the case, but a very material one, as affording space for the vapour of the acid to expand itself in; so as to be, in that state, subject to the action of *light*, a thing of which I had no suspicion before.

Having made a quantity of colourless spirit of nitre, which is readily done by making it boil hastily (in order to prevent too great a loss of the acid) and letting it cool again in the dark, I put different portions of it into several phials, some of them quite full, and others only half full, with every different species of air incumbent upon

them, except the nitrous; which I knew would immediately be decomposed, and give it colour. Then leaving the phials exposed to the light of the sun, in a few days I found the acid in all of them that were only *half full* considerably coloured; whereas, the acid in the phials that were quite full remained as colourless as water.

After having had air of different kinds in those phials which were only partially filled, I contrived to have a *vacuum* above the acid; but still, when it was exposed to the light, it became coloured, as well as that which had air in contact with it*.

I then took some of the phials that were only *half full*, and covering them from the light exposed them for several days to a considerable degree of *heat*. But in that situation they never acquired any colour.

Being now satisfied that it was the action of *light* upon the *vapour* of spirit of nitre that gave it colour, I amused myself with

* In this experiment I used a glass transferrer, executed by Mr. Parker. It is of excellent use when either acids or mercury is employed.

throwing

throwing a strong light, by means of a lens, into the upper part of a phial, the lower part of which contained colourless spirit of nitre. And in this manner I found that I could soon give a strong orange colour to the vapour of the acid; and that, being imbibed by the liquid acid with which it was in contact, *this* also became coloured, first at the top, and then quite through its substance.

In what manner light acts, in order to give colour to the vapour of nitrous acid, remains to be determined; but that it is by imparting *phlogiston* to it, is highly probable, because the liquid acid will become coloured by means of any thing that contains *phlogiston*, independent of light. It remains also to be ascertained, why light has not a similar effect on the vapour of spirit of salt, that of other acids, and vapours of other kinds. But when a sufficient degree of attention shall be excited to the subject, it may not, perhaps, be very difficult to make much farther progress in the investigation

investigation of the chymical properties of light, and its action upon bodies in general. I had been told that the blue colour of the solution of copper in spirit of nitre depended upon light. But when I was engaged in the preceding experiments, I satisfied myself that it did not. For having first taken the most effectual care that the solution was made with the most colourless acid, I examined it afterwards in the night, with the faintest light of a candle; and in several trials always found it as blue as any solution that had been made in the open day-light. I therefore conclude that phlogiston, *as such*, independent of any thing else relating to light, is the thing that gives colour to this acid. That light does contain phlogiston, is, I think, evident from various chymical experiments.

S E C.

S E C T I O N XXIII.

Experiments on the Action of Steam upon several heated Substances.

HA V I N G, since the publication of that paper in the Philosophical Transactions which makes the second section of this volume, made a few other experiments of the same kind with those recited in it, I shall introduce them here, by way of *Supplement* to it. Those already recited relate chiefly to the action of steam upon *charcoal*, or *iron*. I have since extended them to some other substances, and though the results are not very important, it may be of some use to relate them.

Having made another experiment with *iron*, with as much attention as I could give to it, I shall in the first place mention *that*. From two ounces of iron turnings (which is cast iron annealed) I got in the first instance 985 ounce measures of air, with the
loss

loss of 528 grains of water ; and the iron, I found, had gained 292 grains in weight. Then making four ounces of water of this residuum pass over the same iron, it gained four grains more, and all the air that I procured was 998 ounce measures. So that (as extreme accuracy is not to be obtained in processes of this kind) it may be said that two ounces of this kind of iron will, in this way, yield 1000 ounce measures of air ; whereas by solution in vitriolic acid it would have yielded about 800. Consequently more air by two fifths may be procured by this new mode of treatment, which alone should recommend it to those who fill balloons.

Having recommended this process as the cheapest and the most convenient for filling balloons, especially when tubes of cast iron should be made use of ; I was willing to make a trial of one, in order to form some judgment how long they would last for the purpose. I therefore procured one of an inch and a quarter in diameter, and not more than a quarter of an inch in the thickness of

of the metal; and making the middle part of it red-hot, sent steam through it; and from the result of four or five processes with the same tube, I have little doubt, but that, if they were made of the thickness of half an inch in the metal, and care was taken to coat them on the outside with clay and sand, the same tube might probably serve twenty times. That the reader may form some judgment as well as myself, I shall mention the result of my observations.

I heated the tube four or five different times, and in each process transmitted as much water through it as would have been more than sufficient to decompose all the iron that it could have contained. At first four ounce measures of water procured 180 ounce measures of inflammable air, and then six ounce measures procured only 160 ounce measures. I then examined the tube, and found that when both the inside and outside were well scraped with a sharp instrument, it had lost twenty-five grains in weight. The scales from the outside weighed 282 grains, while all that I could get from the
inside

inside weighed only 36 grains. Consequently the tube had gained in weight 28 $\frac{3}{4}$ grains. After this I heated it again, and transmitted through it six more ounces of water, which yielded only 60 ounce measures of air.

From these experiments it may be inferred, that the tube would soon have ceased to give any air; the inside being changed into some depth by the action of the steam, and yet it was not much disposed to exfoliate. In time it would, no doubt, have become brittle, and might be in danger of breaking, from its disposition to *bend* in the course of the process. This bending was very considerable; but did not seem to arise from any tendency in the iron to *melt*. Perhaps by turning it in cooling, this bending, and consequently the danger of cracking after much use, might be prevented; or this property of bending might in a great measure cease, when the metallic state of the tube was destroyed; and yet with care might continue a firm and compact tube, and as fit for this process as at the first. If this should be the case (which experience

experience alone can determine) it is not to say how long a tube of this kind might last. It would then be a kind of *earthen tube*, of the most perfect nature, completely air tight, without being subject to rust or decay.

Upon the whole, should the fondness for balloons be resumed, I see no reason why far the greatest part of the expence attending the filling of them might not be saved by means of this process. A complete apparatus for it would not cost half so much as the filling of a single balloon, that would carry a man, in the common way, and would serve at least a considerable number of times, with the expence of a very few pounds each time; as there would be hardly any thing to pay for besides *fire*, and *attendance*, for a few hours. For such *iron* as would best answer for this purpose might, in most places, be had for a mere trifle. One apparatus, conveniently fixed, might serve for a whole town or neighbourhood.

Having been able to decompose iron by converting it into scales, I also found that
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in this way, I could readily procure *flowers of zinc*, as well as inflammable air from that metal. The flowers came over in a very attenuated state, the air being loaded with them. It might be possible, however, to contrive an apparatus to collect them.

Brass being made with a mixture of zinc and copper, and *zinc* being so easily decomposed by steam, whenever a copper tube is ordered for the purpose of these experiments, particular care should be taken that there be no mixture of brass in it, though it is difficult to have copper cast smooth and solid without a mixture of either brass or tin, which is not much better. Even pure copper tubes become brittle, and at length crack in these experiments.

Having at one time been persuaded to have a little brass mixed with the copper in one of these tubes, I consented that the smallest quantity that could be supposed to be necessary to make the tube compact, should be put into it. But notwithstanding this, and though the tube had a quarter of an inch thickness of metal, it fell to pieces in the
very

very first experiment, in which I sent the steam of no more than two or three ounces of water through it. Inflammable air was produced very copiously, and the flowers of zinc were mixed with it; so readily did the steam separate the zinc from the copper, though the heat was only sufficient to make the tube red hot, and was far from melting it.

Lead would probably be far less affected than copper in these experiments; but then it will not bear a read heat without melting. I made the steam of about four ounces of water pass over four ounces of melted lead, in an earthen tube, with hardly any sensible effect. The loss of water was not more than 0.2 of an ounce measure.

After using *charcoal* in the experiments recited in the preceding section, I went through one process with *coak*, or the cinder of pit coal, and found that from 174 grains of coak, and with the loss of 528 grains of water, I got 1700 ounce measures of air, of which one fifth was fixed air, and 30 ounce measures of it weighed ten grains less than

an equal bulk of common air. The analysis of this air will be found in the section appropriated to that subject.

On *iron ore* this process had no effect. The same was the case with the trial I made of *quick lime*, and such would probably be the case with dephlogisticated earths in general.

With *manganese*, however, the result was different. Having made the steam of four ounces of water pass over 828 grains of this substance, which had been exposed to a strong heat in an earthen retort some time before, I got 35 ounce measures of air, almost the whole of which was pure fixed air, with a residuum a little better than common air. The manganese had lost 132 grains, and from being *black*, was become very *brown*. Again, I transmitted the steam of eight ounces of water over two ounces and an half of manganese, and got about 100 ounce measures of pure fixed air, with a residuum a little phlogisticated. The manganese had lost 112 grains.

SECTION XXIV.

Experiments relating to Iron.

THE most important of all the *metals* is unquestionably *iron*, and it seems to be that which is as yet the most imperfectly understood, and in the manufacture of which there is the greatest room for farther improvements. But as all experimenters in air make use of it, at least for the purpose of procuring inflammable air (that being the cheapest process by which they can make it) it falls daily under their observation, and in consequence of this it may be hoped, that we shall gradually gain a more perfect knowledge of it, and that this knowledge will lead to improvements in practice. I cannot promise much with respect to my own observations on the subject, but all *new facts* ought to be reported, and some that have occurred to me are of such a nature,

as, in the hands of manufacturers in a large way, may perhaps be of some utility.

One of the greatest differences with respect to iron is between that which is called *cast iron* (which is the state in which it comes from the furnace) and that which is *malleable*. In the former state, though exceedingly hard, it is brittle; so that it can no more be brought into any other form without melting, than glass, but when melted, it still continues iron. In the malleable state, it is no longer capable of being melted again, and continuing to be iron, but it is flexible, and without melting, may be hammered into any shape that is wanted.

I had formed an idea, from what I had formerly read on the subject, that this great change in iron was made by nothing more than violent *hammering* while it was red hot; but when I saw the real process, I was soon convinced that this was a mistake. For in the finery furnace (or that furnace which is intended to refine the iron, and make it malleable) the cast iron is exposed to a strong blast of air, while it is red hot,
or

or rather in a half melted state ; and it is this blast of *air* that is the most necessary, if not the only necessary circumstance, in producing this change. Also, while the iron undergoes this operation, a quantity of liquid matter runs from it, and when it is cold acquires the form of a black cinder ; which, from the name of the furnace out of which it flows, is called *finery cinder*. The nature of this substance, I think, I have investigated. When this finery cinder is separated from the common mass, what remains of the iron is actually malleable ; but being in loose spongy masses requires the hammer to consolidate it, and to reduce it into a proper form for farther operations. Also, a considerable quantity of finery cinder remains in the interstices of the spongy masses, and the hammer is useful in separating it.

A farther remarkable change in the constitution of iron, is that by which it is converted into *steel*, which is made by *cementing* it with charcoal. It is then capable of being made elastic, and of receiving various

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degrees of hardness, according to the suddenness with which it is cooled after it has been made red hot. It is also then capable of being melted, without losing its metallic character.

At Birmingham, I find it is common to subject cast iron to this process of cementation; and then it is said to be *annealed*. This gives it a slight degree of flexibility, so that it may be used for some kinds of nails. Having an opportunity of procuring these nails, both before and after annealing, I have made many experiments on them, and find that this process makes a remarkable difference in the chymical properties of the metal; and such as I should not have expected *a priori*.

Iron that is annealed, as well as that which is malleable, and also steel, is readily dissolved in diluted oil of vitriol, and leaves very little residuum; whereas the cast iron that has not been annealed, dissolves with great difficulty, and when it is dissolved, leaves a great quantity of black residuum. The solution itself is also attended with
some

some appearances of a very striking nature, which I shall describe as they occurred to me.

From twenty-four grains of cast iron, in the form of nails, I got, by oil of vitriol diluted with about three times as much water, in the course of some weeks (for without external heat less time is not sufficient) 69 ounce measures of inflammable air, exceedingly offensive to the smell. The nails still retained their form, and as after repeatedly heating the mixture till it boiled, very little more air came from them, I examined them, and found them to be pretty soft, so that I could easily cut them with a knife. They had also the same highly offensive smell with the air that had come from them; and being put into a jar of common air, they soon diminished, or phlogisticated, it.

When these nails were dry, they were brittle; and though white on the outside, they were black within; and after being hastily dried, they appeared to have gained fifteen grains. This experiment explains

what I am told is commonly observed by those who put iron pipes into pits, in which there is water impregnated with vitriolic acid. For in time they become quite soft, or, as they call it, *rotten*, so that they can be cut with a knife. That a good deal of all the constituent parts of iron remains in this substance, was evident from the result of my melting a piece of it with a burning lens. For though it neither gained nor lost any weight, it spirted a little in the fusion, and threw out sparks, just like cast iron in the same experiment.

I was too hasty, however, in concluding that the process above mentioned was over, as the next experiment will shew. Half an ounce of cast nails had been kept in oil of vitriol, diluted with three times as much water, about three weeks, and though the menstruum had been changed several times, and they had yielded 130 ounce measures of air, they retained their form. But I then perceived some specks of black matter floating in the liquor, and after a few days more, I found one-third of the phial in which
the

the solution was made quite opaque with them. Nothing then remained in the form of a *nail*; and during this breaking up of the nails, the air had come from them much more copiously than it had done for a considerable time before. For the whole quantity of air was now 190 ounce measures.

The air, however, comes pretty readily at the beginning of this process. For 60 ounce measures were procured in a very few days, whereas that quantity was not got in as many weeks afterwards, till the form of the nails was destroyed. The black powder that remained after this process had a very offensive smell; and when dried hastily, weighed 18 grains. From another equal quantity of these nails this black powdery residuum weighed seventeen grains and a half, and this was the usual proportion of its weight to that of the iron.

What this *black matter* really is, how the process of annealing can so greatly lessen the quantity of it, and what connexion this circumstance has with the solubility of the iron in acids, are questions that well deserve

serve consideration; and I think it very possible, that a little attention to the subject may lead to an easy solution of them, and a farther insight into the nature of iron.

Another phenomenon in the solution of this cast iron being exceedingly remarkable, and with respect to myself, an *unique* (for I have never been able to repeat the observation) I shall mention the particulars exactly as they occurred to me, and as I noted them in my register at the time.

In the course of last winter, I had put half an ounce of cast iron nails into a mixture of oil of vitriol and water, and in the space of about two months, in the cold weather, they had yielded 72 ounce measures of air; but the menstruum not having been changed, much copperas was con- creted among the nails. In the following spring, when I began to give more attention to the process, I changed this liquor, and occasionally applied to it the heat of a candle, in order to accelerate the solution. A few days after this I observed in the liquor a great number of *transparent fila- ments*,

ments, a quarter, or half an inch in length, like the finest hairs, part of them adhering to the nails, at the ends, but the greatest quantity of these *hair-like crystals*, as they may be called, were detached from them, and floated in the liquor.

Taking some of them out of the menstruum, I found that they dissolved in fresh water, but not immediately. I then took them all out of the phial, and to the same nails put fresh oil of vitriol and water; and in a few days more of these crystals appeared, but not so many as before. On heating the liquor while they were in it, they all disappeared; but after a few days they appeared again, and kept increasing very much. I made them disappear a second time in the same manner, and with the return of cold they appeared again. Having poured off this liquor, with the crystals in it, I heated that liquor, and made them disappear, but they re-appeared in the same liquor when it was cold, and no more in the fresh liquor that was put to the nails. After this the nails broke up, and the air
came

came very rapidly, as in the former process, and, in all, these nails yielded 163 ounce measures of air.

As these crystals were formed a little time before the nails broke up, I imagined that, whatever the substance was, it was that which had held them together, preventing their easy solution, and that it was attracted from the iron in the operation of annealing. I also thought, that by repeating this process, and watching it a few days before the breaking up of the nails, I could not fail to find the same appearance. But though I repeated the experiment, and with all the variations that I could think of, I was never gratified with a second sight of these beautiful crystals.

As these crystals were evidently of a saline nature, I concluded they were only martial vitriol, though in an unusual form. To ascertain this, I dissolved all the fragments I could collect of these crystals in water, and set it to evaporate. But instead of finding any green vitriol, there was only a black incrustation left on the evaporating vessel.

There has been some difference of opinion among chymists concerning the proportional quantity of air yielded by *iron* and *steel*. Having taken some pains to ascertain these facts, at the same time that I was dissolving iron and steel for other purposes, I shall here mention the result of my observations respecting the air that is yielded by all the kinds of iron that have fallen under my notice. I repeated the experiments several times, but the results often varied, without my being sensible of any change in the circumstances of the experiments.

What quantity of air came from *cast iron* in the experiments mentioned above, I have noted in the account of them; but as I frequently repeated the experiment, especially with a view to find the hair like crystals that I have mentioned, I shall here put down the different quantities of air, of which I also took an account in some of them, as well as of the residuums of black matter, which I observed at the same time. At one time a quarter of an ounce of cast
iron

iron gave ninety ounce measures of air, half an ounce gave 163 ounce measures, and two ounces gave 727 ounce measures, leaving a residuum of black powder, which weighed ninety-eight grains; and four ounces of the iron gave a residuum of 163 grains. Upon the whole, therefore, a quarter of an ounce of this iron may be said to give ninety ounce measures of inflammable air.

Of the *annealed cast nails*, a quarter of an ounce gave at one time 101 ounce measures of air, at another time 105½, at another 106, and the most of all 107. Half an ounce gave 212 ounce measures. Half an ounce of iron turnings, which is iron less perfectly annealed, gave ninety-six ounce measures of air. It is evident, therefore, that the annealed cast iron gives considerably more inflammable air than that which had not been annealed; and this is easily accounted for, from the much greater quantity of residuum from the iron that has not been annealed, which residuum, it will be seen, contains much phlogiston.

A quarter

A quarter of an ounce of *steel* gave ninety ounce measures and an half of air, $118\frac{1}{2}$ grains gave ninety-five ounce measures and an half of air, and the same weight of the very same iron from which that steel had been made gave ninety-six ounce measures. Half an ounce of steel gave 207 ounce measures, and the black residuum, though greater than is found in the solution of iron, or cast iron annealed, was too small to be weighed. At other times, however, it has been much more considerable, though I have no note of the exact quantity that I found in any experiment. It is also observable, that in the solution of this half ounce of steel, in which there was so little residuum, the quantity of air was unusually great. And, I presume, that the differences in the quantities of air from given weights, both of steel, and different kinds of iron, will always be in some reciprocal proportion to the quantity of this residuum, which I have never found any solution of iron to be entirely without; and this will account for the different quantities of air that different persons

persons have reported to have found both in iron and in steel. As a greater quantity of inflammable air was procured from annealed cast iron, than from malleable iron, I think it may be concluded, that steel (which is malleable iron annealed) will always give more air than iron, provided that, in the solution, it should yield no greater a quantity of black residuum. But in general there is much more of it found in the solution of steel than in that of iron.

This black powder which remains from the solution of iron or steel, and especially that from the cast iron not annealed, which is in such great quantity, well deserves to be thoroughly examined. Having made a few miscellaneous experiments upon it, I shall here give the result of them. Putting eighteen grains of it into spirit of salt, there remained eight grains undissolved. This was of a grey colour, but being sprinkled upon hot iron, it did not appear, by this test, to contain any *sulphur*.

I exposed a quantity of it to the heat of the burning lens in vacuo, and found the air that
was

was expelled from it to be one ninth fixed air, and the rest inflammable, of the explosive kind. Ten grains of the powder gave nine ounce measures of air; but a good deal of this light powdery substance was unavoidably lost in conveying it through the water after the process.

This matter also appears to contain something of iron, though it is no longer soluble in oil of vitriol. For being exposed to the heat of the lens in open air, it was melted, and weighed just the same before and after fusion, which had also been the case with that cast iron which had been imperfectly dissolved in oil of vitriol, and retained its form.

From the air which this black powder gave, it may be concluded, that it contains much *plumbago*; but that which remains after the solution of malleable iron, or steel, is more nearly, or perhaps perfectly so. Nineteen grains of this (though it is possible that some of the other might have been accidentally mixed with it) was reduced to six grains, by being melted with

the heat of the lens in the open air, and then became a glass, or slag; which was nearly the same result that I had from genuine plumbago, the greatest part of which is reduced into fixed and inflammable air.

The process of *cementation* is not, I am persuaded, sufficiently understood. It is the opinion of those who make steel, that the metal neither gains nor loses weight in the process. Those who anneal cast iron tell me, that it loses considerably in that process, which is similar to that of making steel, though the iron is taken in very different states. On the contrary, I found both malleable iron and cast iron to gain a little weight by cementation in my fires. Seventy-two grains of iron wire gained three grains, and became of a dark black colour, and three ounces of cast iron nails gained six grains. But perhaps the heat that I used was too great for the purpose. For in consequence of this iron will attract water, and become in part *scales of iron*, which is always attended with an increase of weight; whereas in the same process with

with a long continued and moderate heat, it is very possible that some of the elements of that black matter, or plumbago, may go out of the iron, and join the charcoal, forming perhaps a sulphur, which may be sublimed, and dispersed in the process.

I have observed that I once expelled from some annealed cast iron nails such inflammable air as constituted fixed air, when it was decomposed with dephlogisticated air, which shews that it had imbibed some of that kind of inflammable air which is peculiar to charcoal. But when I dissolved that kind of iron in vitriolic acid, the air was the very same with that which came from malleable iron, no fixed air being produced in the decomposition of it.

SECTION XXV.

Experiments on Finery Cinder.

THE most useful of my observations on the subject of *iron* relates to the nature of the *finery cinder*, the substance which, I have observed, runs in a liquid form from cast iron in the process of converting it into malleable iron. This has generally been considered as a thing of no value; and is commonly thrown away as such, though my brother-in-law, Mr. WILKINSON, has, with advantage, made use of a certain proportion of it in the smelting of iron. I flatter myself, however, that by a due attention to some observations which I shall here relate, this substance may hereafter be employed to more advantage. But before I mention this use of it, in reducing it to iron, I shall recite some experiments which I made with a view to ascertain the nature of it.

The first thing that occurred to me, was to attempt the solution of it in the several mineral acids, which I did with the following results. Putting 60 grains of it, finely pulverized, into each of the three acids, and leaving them two or three days to digest; of that which had been in the vitriolic, and also of that which had been in the marine acid, there remained 15 grains undissolved; but of that which had been exposed to the nitrous acid, 40 grains. Another time, I observed that of 74 grains exposed to the spirit of salt, 30 remained undissolved.

This substance, as it comes from the common furnace, is seldom without some portion of iron, as appeared by melting it in the fun, when 24 grains of it gained one grain in weight. It also gained a little weight when I exposed it to the action of steam in an hot earthen tube.

Finery cinder, when hot, readily unites with most kinds of earth, greatly promoting their fusion, in the same manner as the *fluor*, which has its name from that circumstance. Attempting to get air from the finery cinder

in an earthen retort, 70 ounces of it yielded 40 ounce measures of air; but as the retort was dissolved, and most of the contents run out, nothing certain could be pronounced concerning the origin of this air, of the last portion of which three-fourths were fixed air. The standard of the residuum was 1.67. In one part of it a candle was extinguished, but another portion was slightly inflammable.

The circumstance that first gave me an insight into the real nature of this finery cinder, was my having found a piece of malleable iron become throughout a perfect finery cinder, in consequence of being left in the open fire of my furnace, when the heat had been very great. The appearance of it alone satisfied me, that it was the same thing; and comparing it in a variety of processes with real finery cinder, I found that they did not differ in any respect. They appeared also to be the same substance with the *scales* which fall off from iron when it is heated in an open fire, with that which malleable iron is brought to by being melted

melted in the open air, or in dephlogisticated air, and also by being exposed to the action of steam when it is red hot. In all the different processes recited in my paper on phlogiston, it was perfectly indifferent which of these substances I made use of, and therefore, I omit to mention the particulars.

Finery cinder is therefore to be considered as iron increased about one third in weight by imbibing pure air, or rather water, though with the loss of its own phlogiston. Nothing, therefore, is wanting to bring it to the state of iron again, but the expulsion of the pure air or water, with which it is saturated, and giving it the phlogiston which it has lost: and this is readily done by heating it in close vessels, in contact with any thing that contains phlogiston. This is effected in the most complete manner, by heating it with a burning lens in inflammable air. But it is likewise done by cementation with charcoal, with coak from pitcoal, or with raw coal. In all these processes, the finery cinder loses about one third of its

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weight,

weight, and is then perfectly soluble in acids, and attracted by the magnet. Consequently, it is *perfect iron*. But whether it can be made useful iron, and in a manner so cheap as to make it worth the while of the manufacturer of iron, to establish any work for the purpose, is not for me to say. I would only observe, that iron thus made from finery cinder in my retorts is not in the state of simple *cast iron*, but of that which is annealed, and likewise partially malleable.

SEC.

SECTION XXVI.

Of Air acting through a Bladder.

ONE of my former experiments which I was least able to account for, was the diminution of nitrous air in a bladder swimming at liberty in a trough of water; the consequence of which had always been, that in a few days the nitrous air was diminished to about one fourth, and this was phlogistified air. All the progress that I had then made in the investigation of this curious fact, was finding that it depended, as I then thought, upon the bladder being kept alternately dry and moist; because when the bladder was kept covered with water, it remained full, and the air within it was not changed. This was also the case when the bladder was kept dry. But I did not consider that when the bladder was kept under water, there was no *air* in contact with

with it; and I did not then suspect that this change in the air depended on the action of the nitrous air upon the external common air through a moist bladder; though I had found that coagulated blood has a power of acting upon air, and is of course liable to be acted upon by air, through any bladder.

At length, suspecting that this *might* be the case, I made the following experiment. Taking a bladder which contained twenty ounce measures of nitrous air, and tying it very tight, I introduced it into a glass jar, which contained forty ounce measures of common air; because, in that proportion, they would be able, if they had any mutual action, to saturate one another. Wishing at the same time, to observe the changes that might gradually take place in each of the kinds of air, I examined them both at different periods.

The process was begun on the 18th of May, and on the 21st I found that there were only 34 ounce measures of the common air, and eleven of the nitrous, the bladder being quite sound; so that it was sufficiently
evident

evident, that the two kinds of air, had affected each other through the substance of the bladder. On the 25th of the same month there were $31\frac{1}{2}$ ounce measures of the common air, and four and a half of the nitrous; and examining the state of both of them, I found the standard of the common air to be 1.8, which was a state very near that of extreme phlogistication; and that of the nitrous 1.7. That is, equal measures of this and of common air, occupied the space of 1.7 measures, which shews that it had almost lost its power of affecting common air, or to express myself perhaps more correctly, there was but a small proportion of nitrous air in it.

On the 8th of June I examined them for the last time, after having observed no farther change for some days in the quantity of the common air (as indicated by marks which I had made on the outside of the jar) and I found only 28 ounce measures of the common air, of the same quality as when I had examined it before, viz. of the standard of 1.8, and only three ounce

ounce measures of the nitrous air, which did not affect common air at all. Neither of them contained any portion of fixed air, and both of them extinguished a candle.

Nothing now remained to my complete satisfaction, with respect to my former observation of the diminution of nitrous air, contained in a bladder. But I farther wished to satisfy myself with respect to the action of *inflammable air*, on either common or dephlogisticated air, in the same circumstances. Nitrous air affects pure air by simple contact, without ignition; whereas, inflammable air, I had observed, has very little effect upon pure air when they are simply mixed together. I was, therefore, surprized to find that inflammable air has a very considerable action upon dephlogisticated air through a bladder, without any assistance from heat; and moreover, that the union of these two kinds of air, thus produced, forms fixed air. The experiments which I made for this purpose, were as follows.

Into

Into a jar containing 123 ounce measures of dephlogisticated air, I introduced a bladder, containing 23 ounce measures of inflammable air; and after a few days, I observed that the bladder in which it was contained was become a little flaccid. After about three weeks, I examined both the kinds of air, and found that the bladder contained only two ounce measures, and that this was no longer inflammable, but extinguished a candle, though it had in it a mixture of pure air. The air within the jar then contained one twentieth of its bulk of fixed air. The dephlogisticated air was diminished seven ounce measures; and from being of the standard of 0.5, with two equal measures of nitrous air, it was now become of 1.4. The bladder had a slight smell of putrefaction, but it was perfectly air tight.

It is observable, that in this experiment part of the dephlogisticated air had passed unchanged into the bladder of inflammable air, whereas the inflammable air which had passed through the bladder into the dephlogisticated air, had united to it, and formed

fixed

fixed air. The transmission of the dephlogisticated air through the bladder was much more remarkable in the following experiment.

Having introduced a bladder filled with inflammable air into a large jar of dephlogisticated air, the bladder, after two days only, had in it a great mixture of dephlogisticated air, and was as much distended as when it was first put into the jar. A quantity of it exploded exactly like a mixture of one-third dephlogisticated, and two-thirds inflammable air. The bladder was perfectly sound and sweet, and the dephlogisticated air was not sensibly altered.

Again, having introduced a bladder containing ten ounce measures of inflammable air into a jar containing 100 ounce measures of dephlogisticated air, of the standard of 0.3, I found, about a month afterwards, that the air in the jar was diminished to 90 ounce measures, and the inflammable air to five ounce measures and an half. The quality of the air in the bladder and of that in the jar was very nearly the same, though the bladder

bladder was perfectly sound and sweet. The air in the bladder, with equal measures of nitrous air, was of the standard of 0.76, and that in the jar of 0.74. Both of them also contained a small portion of fixed air. In this case, therefore, both the kinds of air had not only been transmitted through the bladder, but some decomposition had also taken place within it, as well as within the jar.

In another experiment of this kind, both the bladder of inflammable air, and the jar of dephlogisticated air, after some time, contained each of them a portion of fixed air, and likewise both the kinds of air unaffected by each other. For both of them exploded when they were examined separately.

I had some suspicion that the fixed air in this process might have come from the putrefaction of the bladder; especially as in one of the cases the smell of it was very sensibly putrid. To determine this, I put a bladder filled with inflammable air into a jar containing inflammable air likewise, and

let

let it continue there till it was quite putrid, and dissolved. But after this I found no sensible quantity of fixed air in the jar, I therefore concluded, that the fixed air which I found in the preceding experiments, came undoubtedly from the union of the inflammable and dephlogisticated air, in consequence of their acting upon each other through the substance of the bladder.

It seems to follow from these experiments, that fixed air is really *formed* when inflammable air of charcoal, &c. is exploded together with dephlogisticated air; and also that the greatness of the heat prevents its formation, when inflammable air from metals is used. For though, in the explosions with the electric spark, no fixed air was produced from the decomposition of the purest inflammable air, it was evidently so with the same kind of inflammable air in these experiments with a bladder, in which no heat is used.

SECTION XXVII.

MISCELLANEOUS EXPERIMENTS.

1. *Of the Influence of Water on Phlogisticated Air.*

IN the infancy of my experiments I concluded that all kinds of air were brought by agitation in water to the same state, the purest air being partially phlogisticated, and air completely phlogisticated being thereby made purer; inflammable air also losing its inflammability, and all of them being brought into a such a state, as that a candle would just go out in them. This inference I made from all the kinds of air with which I was then acquainted, and which did not require to be confined by mercury, being brought to that state by agitation in a trough of water, the surface of which was exposed to the open air; never imagining

that when the air in my jar was separated from the common air by a body of water, generally about twelve inches in depth, (adding that within to that without the jar) they could have any influence on each other. I have, however, been long convinced that, improbable as it then appeared to me, this is actually the case, though a fact so remarkable well deserves to be farther attended to.

As I have no where recited any experiments which prove that this change in the state of the air, agitated in water, depends upon that water having some communication with the atmosphere, I shall do it here, after an account of some other experiments, from which it is equally evident that, notwithstanding every possible precaution, the air will be changed if there be that communication.

I agitated, a quarter of an hour, about three ounce measures of air, phlogisticated with nitrous air, in a vessel containing 20 ounces of water, which had been boiled several hours, and while it was very warm ;
after

after which it was diminished one-sixth, and was so much improved, that the standard of it was 1.74. The next day I agitated the remainder of it another quarter of an hour in the water which had been boiled at the same time, when it was diminished about one-tenth, and the standard of it was 1.57.

I also agitated the same quantity of air, phlogisticated with iron filings and brimstone, in the same manner twenty minutes, when it was diminished one-seventh, and the standard of it was 1.66.

I afterwards expelled a little air from the water in which this air had been agitated; and from that with which the first of these experiments was made, I got air, the standard of which was 1.66; and from the water in which the second experiment was made air of the standard of 1.33. In this a candle would have burned. It is evident, from the diminution of the air in these experiments, that part of it is absorbed by the water. But it is no less evident, as will appear by the next experiment, that the place of

some part of the air which had been absorbed was supplied by a purer air, which the water had imbibed from the atmosphere. That water does imbibe the purer part of atmospheric air, in preference to that which is impure, is evident from any examination of it. For if the water be clear, and free from any thing that is putrescent, the air expelled from it by heat is generally of the standard of 1.0; whereas that of the atmosphere, when the nitrous air is the purest, is about 1.2.

But the experiment which decisively proved that my former conclusion was fallacious was the following. After expelling all air, by boiling, from a quantity of water, I put to it, in separate phials, air that had been phlogisticated with iron filings and brimstone; and corking them very well, I left them with their mouths in water, and I occasionally agitated them. After about two months I examined them, and I found both the air that was confined by the water, and also that air which by heat I expelled from the water, completely phlogisticated.

gisticated. The same was the result of a similar experiment, in which the air had been expelled from the water by the air pump.

2. *Of Air produced more or less rapidly.*

IT was one of my first observations on the subject of air, that, in several cases, a much greater quantity of air was produced by a rapid, than by a slow process. I have several times verified this fact, though I cannot very satisfactorily account for it. In my late experiments I had an instance of it, which it may be proper to recite.

From an ounce and an half of whiting in a gun barrel I got, with a sudden and great heat, 240 ounce measures of air, of which some portions contained nine tenths of fixed air, and the residuum burned with a lambent blue flame. Repeating the same process very slowly, I got no more than 150 ounce measures of air from the same quantity of whiting. And again repeating the process with a greater and a more sudden

*if it escapes
if it is slow
before it*

heat than at first, I got, from the same quantity of materials, 279 ounce measures of air.

3. *Of different kinds of Air remaining diffused through each other without uniting.*

IN my former publications I observed that, when any two kinds of air, of the greatest different specific gravities, and without any known cause of attraction to each other, are mixed together in the same vessel, they are never separated by mere rest, but continue equally diffused through each other. This at least I found to be the case with respect to all the kinds of air that can be confined by water, and several of those that only can be confined by mercury, except that *inflammable* air always remains *separate* from acid or alkaline air, though contained in the same vessel. I had lately a very clear proof of this with respect to dephlogisticated and inflammable air.

Having filled the strong glass vessel, fig. 5, in which I usually fire them by the electric

tric spark, with the usual mixture of one third dephlogistated and two thirds inflammable air, I let it remain in a perpendicular position all night, in which there was certainly time enough for them to have separated from each other, if their very different specific gravities would have disposed them to do so. But though the two wires between which the explosion is made were at the very top of the vessel, which in the case of a separation would have been the region of pure inflammable air (which cannot take fire of itself) yet when the spark passed between them, the two kinds of air were exploded as completely as they could have been immediately after they had been mixed.

It is also observable, that the phlogistated and dephlogistated air which compose the atmosphere are of very different natures, though without any known principle of attraction between them, and also of different specific gravities; and yet they are never separated but by the chemical attraction of substances, which unite with the

one, and leave the other, as in all those processes which I have termed phlogistic.

4. *Of the Ice of Water impregnated with Vitriolic Acid Air.*

IT is remarkable that water impregnated with vitriolic acid air retains all its air when it is frozen, though every other kind of air (if the liquor containing it can be frozen at all) is separated from it in the act of freezing. I have now farther observed, that this ice sinks in the liquor from which it is frozen, in which it resembles the ice of oil. This is a fact which I barely mention, without having any theory to account for it.

5. *Of the Precipitate from the Solution of Metals by Heat.*

IN my fourth volume of experiments I have recited many on liquors exposed to a long continued heat, in glass tubes hermetically sealed, and among other things observed a remarkable deposit from the solution

lution of *mercury*, and also that of *copper* in spirit of nitre. But they both require a considerable *time*. I have since found that the deposit from *iron*, and from copper in volatile alkali, are made much sooner.

In a glass vessel hermetically sealed, a good deal of iron was precipitated in the form of *red earth* from a weak solution of it in spirit of salt, placed in a sand heat, only a single day. It was also precipitated in great abundance from a solution of spirit of nitre in the same time. In this case the vessel was not quite closed, so that a little could evaporate. The remaining liquor was quite colourless.

Copper was precipitated in the form of a deep blue earth, from a solution of it in volatile alkali at the same time; but the remaining liquor was almost as blue as at first. The solid precipitate smelled strong of the volatile alkali, as well as the solution itself.

6. *Of heating Iron in fluor acid Air.*

HAVING mentioned the effect of heating various substances in different kinds of air by means of a burning lens, that I may not omit to mention any thing that I have observed of the kind, and that I can think any person might wish to be informed of, I shall, in this miscellaneous section, recite the following experiments.

Throwing the focus of the lens upon iron turnings in fluor acid air, a dense white fume presently filled the vessel, and during the process the heat was remarkably great, so that I could not touch the upper part of the glass vessel in which the experiment was made. The quantity of air was considerably diminished, and a quantity remained that was not absorbed by water, but not enough to examine it.

7. *Of the Solution of Precipitate per se in Spirit of Salt.*

PRECIPITATE *per se* dissolves with great rapidity in spirit of salt. A quantity of this solution I impregnated with nitrous vapour

vapour, on which the surface of the fluid, and the sides of the phial, were instantly covered with crystals, larger than those of the precipitate. The colour of the liquor was then of a light blue, or green. Afterwards it assumed a deep brown, inclining to yellow, and a great quantity of white matter was deposited, occupying almost the whole space of the liquor. This must, I presume, have been corrosive sublimate, the pure air from the precipitate having dephlogisticated the spirit of salt, which is a necessary circumstance in this preparation.

8. *Of Air extracted from the Water that is useful in fertilizing Meadows.*

IT has been found that meadows watered with a fresh spring produce abundance of grass without any manure. Having had reason to think that phlogiston, administered in some form or other, is the food of plants, I imagined that this water, being in itself pure, and free from all putrescent matter, must contain phlogisticated air, especially as I had been told that the water is only
useful

useful after its first issuing from the earth, and that the first meadows it overflows are those which receive the most benefit from it.

I therefore desired Mr. YOUNG, to whom this country owes so much for the attention that he has given to the business of agriculture, to send me a quantity of such water as had been found to be most useful in fertilizing meadows. He was so obliging as to send me two bottles of such water; and as soon as I received them I expelled from them the air they contained, and found that which was in one of them, sufficiently impure, the standard of it being 1.4; but that in the other, was such as is usually contained in water that has been long exposed to the atmosphere, being of the standard of 1.0; so that I imagined it had not been sufficiently well corked. After the water in the other bottle had been exposed a few days to the open air, the air contained in it was of the same quality. Upon the whole, I think that the experiment was sufficiently favourable to the hypothesis, but more ought to be made, and if possible, on the spot.

9. *Of*

9. *Of Water heated in Gun Barrels welded up.*

AS inflammable air was produced in some experiments, in which I endeavoured to change the nature of water by making it red hot in a gun-barrel, the orifice of which was welded up, it may not be improper just to mention them in this place. They will likewise serve to shew the expansive force of water in that state. The experiments were made in March 1783.

Putting 16 grains of water into a gun-barrel, containing $4\frac{1}{2}$ ounce measures, I got it welded up; and making it red hot, it burst in the middle after a few minutes. I ascertained the quantity of water, by putting it into a small glass tube, which I sealed hermetically, and put within the gun-barrel.

I then put six grains of water into the thicker half of a musket barrel, and three grains and a half into a thinner barrel. These did not burst when they were red hot, and being pierced under water, inflammable air rushed out. I repeated these experiments, and

and always had the same result; inflammable air being procured, when the gun-barrels were opened under water; and if the water was in sufficient quantity, part of it at least (for I could not measure it with exactness) was found in the barrel.

10. *Of the Solution of Iron in phlogisticated and dephlogisticated nitrous Acid.*

THAT the giving of nitrous air depends upon phlogiston, is evident from the phenomena which attend the solution of iron in phlogisticated and dephlogisticated acids. Pouring a small quantity of phlogisticated nitrous acid into a large quantity of water, which had iron wire in it, it presently became of a dark colour; but this was soon precipitated, and the solution assumed a lighter colour. I then poured off the solution, which was of a slight brown colour, and pouring into it more phlogisticated nitrous acid, it immediately became of a very dark colour, and emitted air copiously.

On

On examination it appeared to be strong nitrous air. After the emission of this air, the dark colour disappeared. These phenomena, therefore, exactly resembled those of a solution of green vitriol, which assumes a dark colour by imbibing nitrous air, and becomes clear again by the expulsion of it. The dark green spirit of nitre had the same effect as the brown phlogisticated acid, but the dephlogisticated nitrous acid had no such effect.

It is easy to make a pretty strong solution of iron in dephlogisticated nitrous acid that shall be green and give no air, if it be kept very cold during the process. But if phlogisticated nitrous acid be poured into the solution, it presently becomes very black, and emits air. This blackness will some times, if the nitrous acid be very volatile, go off almost immediately; but in all cases it will do so *in time*, and leave the liquor like water, or with a slight tinge of yellow; owing probably to part of the ochre having imbibed pure air, and thereby tending to become red.

Nitrous

Nitrous air also admitted to a green solution of iron in nitrous acid immediately turns it black, just as it does a solution of green vitriol.

Phlogisticated nitrous acid dropped into a solution of green vitriol also makes it black. The green solution of iron in spirit of nitre, yields very little air by heat, and this is not nitrous air. When charcoal was put into it, and heated, it also gave little or no air.

S E C T I O N XXVIII.

Observations relating to Theory.

IT is always our endeavour, after making experiments, to generalize the conclusions we draw from them, and by this means to form a *theory*, or *system of principles*, to which all the facts may be reduced, and by means of which we may be able to foretel the results

sult of future experiments. With a view to this, it has of late been a great object with philosophers to ascertain the number of *elements* that are necessary to constitute all the substances with which we are acquainted, and especially the different kinds of *air*, to which our attention has been much directed, in consequence of their seeming to bring us a little nearer to the ultimate constituent parts of bodies; finding that by their union they are capable of forming solid masses.

In my former publications I have frequently promised to give such a *general theory* of the experiments in which the different kinds of air are concerned, as the present state of our knowledge of them will enable me to do. But, like Simonides with respect to the question that was proposed to him concerning God, I have deferred it from time to time; and indeed I am more than ever disposed to defer it still longer, as I own that I am at present even less able to give such a theory as shall satisfy myself, than I was some years ago; new difficulties having arisen, which un-

hinge former theories, and more experiments being necessary to establish new ones.

Fluctuating, however, as the present state of this branch of knowledge is, I do not think that I can, on this occasion, intirely decline giving some observations of a theoretical nature; and though I cannot pretend to perform the whole of my promise, I shall, at the close of this volume, give a summary view of what appears to me to be the constituent parts of all the kinds of air with which we are acquainted, and a more particular account of the hypothesis concerning phlogiston, which is at present more an object of discussion than any thing else of a theoretical nature. The sketch that I shall give will at least serve, like our former theories, to amuse us when we look back upon it, after having gained a more perfect knowledge of the subject.

The only kind of air that is now thought to be properly elementary, and to consist of a simple substance, is *dephlogisticated air*, with the addition at least of the principle of *heat*, concerning which we know very little;

and

and as it is not probable that this adds any thing to the *weight* of bodies, it can hardly be called an *element* in their composition. Dephlogisticated air appears to be one of the elements of water, of fixed air, of all the acids, and of many other substances, which, till lately, have been thought to be simple. The air of the atmosphere, exclusive of a great variety of foreign impregnations, appears to consist of dephlogisticated and phlogisticated air.

Till Mr. CAVENDISH's fine experiment on *phlogisticated air*, this was imagined to be a simple substance, and we entertained no hope of being able to decompose it; but in this, he has, to all appearance, happily succeeded, and, very contrary to my expectations, has shewn that it consists of nitrous acid and phlogiston; this acid having always been produced by decomposing it with a sufficient quantity of dephlogisticated air. I see nothing to object to his experiment, and I consider it as one of the greatest discoveries on the subject of air; but I am at a loss to account for some produc-

tions of phlogisticated air, in which no nitrous acid is concerned. At least we get air which has all the known properties of phlogisticated air, as its extinguishing a candle, being fatal to animals, and not being affected either by nitrous air, or by lime water, in heating charcoal and red precipitate, in the decomposition of alkaline air, &c. If the phlogisticated air procured in these processes be resolvable into nitrous acid, like that of which the atmosphere consists, we shall be more embarrassed than ever with respect to our theories concerning air, since the elements of nitrous acid must then be found where they were least expected. Since, however, in my experiments, volatile alkali was produced from nitrous air and iron, the production of phlogisticated air from alkaline air may not be thought extraordinary on this supposition, viz. of phlogisticated air containing nitrous acid.

Perhaps some part of the difficulty may be relieved, by making a little alteration in the hypothesis of Mr. CAVENDISH, and supposing, agreeably to the some experiments

ments recited in this volume, that the *acid principle* is supplied by the dephlogisticated air, while the nitrous air gives the base of the nitrous acid, and phlogiston; and then this air may perhaps be considered as phlogiston combined not with all the necessary elements of nitrous acid, but only what may be called the base of it, viz. the dephlogisticated nitrous vapour, or something which, when united to dephlogisticated air, will constitute nitrous acid.

Fixed air seems to be a compound of phlogiston and dephlogisticated air, without any water, and for this reason, perhaps a red heat may be as requisite to form the union between them, as it is to form that between phlogiston and vitriolic acid, in the composition of sulphur, which likewise contains no water. It is something remarkable, that two substances, so different from each other as *fixed air* and *water*, should be analyzed into the same principles. But there is this difference between them, that *water* is the union not of pure phlogiston but of inflammable air and dephlogisticated air.

Inflammable air seems now to consist of water and inflammable air, which, however, seems extraordinary, as the two substances are hereby made to involve each other, one of the constituent parts of water being inflammable air, and one of the constituent parts of inflammable air being water; and, therefore, if the experiments would favour it (but I do not see that they do so) it would be more natural to suppose that water, like fixed air, consists of phlogiston and dephlogisticated air, in some different mode of combination.

There is an astonishing variety in the different kinds of inflammable air, the cause of which is very imperfectly known. The lightest, and therefore, probably, the purest kind, seems to consist of phlogiston and water only. But it is probable that *oil*, and that of different kinds, may be held in solution in several of them, and be the reason of their burning with a lambent flame, and also of their being so readily resolved into fixed air, when they are decomposed with dephlogisticated air; though *why* this

this should be the case I cannot imagine.

When inflammable air and dephlogisticated air are burned, the weight of the water produced is never, I believe, found quite equal to that of both the kinds of air. May not the *light*, therefore, emitted from the flame, be part of the phlogiston of the inflammable air, united to the principle of *heat*? And as light accompanies the *electric spark*, may not this also be the real *accension* of some phlogistic matter, though it is not easy to find the source of it?

Nitrous air, according to the long established hypothesis, consists of nitrous acid and phlogiston. But if I may be allowed to correct this theory, in the manner mentioned above, we must say that it consists of a dephlogisticated nitrous vapour, and phlogiston; and that the addition of dephlogisticated air, and also of water, is necessary to make it nitrous acid; such at least being no improbable inference from the experiments recited in this volume.

Dephlogisticated nitrous air, I take to be dephlogisticated nitrous vapour only, and therefore, perhaps, an elementary substance, as well as dephlogisticated air. It seems to be produced by depriving nitrous air of its phlogiston. But there are difficulties relating to this subject, which I shall mention presently.

The different kinds of *acid air*, it seems to be agreed on all hands, consist of the peculiar acids, in the form of vapour, and phlogiston; so that vitriolic acid air consists of the same principles with sulphur, neither of them containing any water.

Fluor acid air, which I once thought to be nothing more than vitriolic acid air loaded with a peculiar species of earth, I now concur with others in thinking to consist of a peculiar acid and that earth.

Alkaline air will appear, I think, from my late experiments, to consist of some combination of inflammable air and phlogisticated air, or of something capable of being converted into phlogisticated air, but

the

the mode of combination, and the proportion of the ingredients, remain to be investigated. That water enters into the composition of alkaline air, seems necessary to be admitted, because it is decomposed into inflammable air, which I cannot help thinking necessarily requires water. It seems, however, clearly to be inferred from my late experiments, that there is no occasion to admit the *alkaline principle* into the number of *elements*; the *alkalinity*, as I may say, some way or other arising from phlogiston, or phlogisticated air, as *acidity* arises from dephlogisticated air; and this may throw some light on the affinity between these two principles.

As to the *hepatic air* of Mr. BERGMAN, I have not given much attention to it; but it seems to have been proved by Mr. SENEBIER's elaborate analysis of it, to consist of the same elements with the liyer of sulphur from which it is made.

I shall now speculate more largely on the subject of the *nitrous acid*, and the kinds of air that are procured by means of it, that
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the difficulties attending the subject may be more distinctly perceived, and consequently that our attention may be excited to the solution of them.

As inflammable air fires readily in a phial from which it has displaced nitrous acid, there must be in this acid a *vapour* capable of combining with phlogiston, and which may therefore be called *the dephlogisticated vapour of spirit of nitre*. This vapour is absorbed by water; for if the inflammable air be transferred through water into another vessel, it burns only as usual. May it not be this vapour, loaded with phlogiston (with which it has an affinity) and some little nitrous acid (which, perhaps, cannot be wholly excluded, but which may be considered as an extraneous substance) that constitutes the *nitrous air*, which is produced when metals are dissolved in this acid, and may not one part of this nitrous air, on losing some of its phlogiston, by means of the contact with iron, become what I have called *dephlogisticated nitrous air*, at the same time that the other part, being super-saturated

saturated with phlogiston, becomes phlogisticated air.

As nitrous acid is reproduced when dephlogisticated air is admitted to nitrous air, or, according to this theory, to dephlogisticated nitrous vapour and phlogiston, do not these principles, together with water, which is requisite at least to the forming of an union between them, make nitrous acid. If so, this acid must consist of dephlogisticated air, dephlogisticated nitrous vapour, phlogiston, and water. In the solution of metals in this acid, then, the first and last-mentioned of these four elements, together with part of the third, must unite with the calx of the metal, while the second, joined with the remainder of the third (or which is the same thing with phlogiston from the metal) constitutes nitrous air. I do not need to add that *latent heat* seems to be a necessary ingredient in every kind of air.

On the other hand, on the supposition that the nitrous air itself contains all the necessary elements of the nitrous acid, it may be supposed that when dephlogisticated

and nitrous air are mixed, the former of them, uniting with the phlogiston of the latter, makes that *water* which is necessary to the liquid state of the nitrous acid. But I am inclined to prefer the other hypothesis; because when the two kinds of air are mixed in a vessel of dry mercury, no liquid is produced, and if the surface of the mercury be small, it is a long time before the redness, occasioned by the mixture, disappears.

Iron, or the scales of iron, seem to give phlogiston to nitrous air, which is by that means super-saturated, and becomes phlogisticated air; though this is a process not easy to be accounted for. For why should one part of the nitrous vapour be deprived of its phlogiston, while the other part, contained in the same vessel, and consequently, as far as we can perceive, being in the same circumstances, is more firmly united to it. It may, perhaps, therefore, be more probable, that it is the nitrous vapour in the nitrous air that joins the iron, while the phlogiston alone remains united with the principle of heat; and that this is that which constitutes phlogisticated

gified air, though some nitrous acid vapour may remain incorporated with it, which is absolutely necessary to explain Mr. CAVENDISH'S experiment. The quantity of nitrous acid contained in a given quantity of phlogified air, should be determined, in order to remove this doubt.

The hypothesis I have mentioned is favoured by an observation which I have lately made, viz. that *iron*, which contains phlogiston, decomposes nitrous air much more readily than the *scales of iron*, which contain little or no phlogiston, and especially by the iron gaining weight, in consequence of its being used to produce this change in nitrous air. Indeed, in this process with the scales of iron, the nitrous air is decomposed so very slowly, and imperfectly, that it may be supposed that it is only done by means of some little phlogiston that may remain in those scales, and the known affinity between phlogiston and this vapour, make it more probable that this may be the case.

Still, however, the former difficulty is not quite removed; because part of the ni-

trous

trous air is left without its phlogiston, while the other part remains more firmly combined with it. But then, in a course of time, all the dephlogisticated nitrous air will unite with the iron, and all that is left in the vessel will be phlogisticated air. It seems, therefore, as if the union between the phlogiston and the nitrous vapour must first be loosened before there can be a complete union between that vapour and the iron, and that it is in this intermediate state only that the dephlogisticated nitrous air can be obtained.

Another difficulty attending this explanation is, that in the nitrous air the nitrous vapour is already saturated with phlogiston, and therefore could not be made to quit that, in order to join the same principle in the iron. To this I can only say, that the calx of the iron and the phlogiston together may have a more powerful attraction to the nitrous vapour than its own phlogiston, joined (as it must be in the form of air) with the principle of *heat*. We see, in other cases, that the same principle in one mode of combination

nation will have a stronger affinity to any particular substance than it has in another. Thus all the three mineral acids can never receive so much phlogiston while they are in a liquid form, but that they will dissolve metals, which, in the case of the vitriolic and nitrous acids, is always supposed to be by means of the affinity which those acids have to the phlogiston of metals.

As dephlogisticated nitrous air is in part converted into common air, that is part of it into dephlogisticated air, by the electric spark, and also by heating bits of crucibles in it, does it not appear, that it contains the proper element of this air, as well as it does that of nitrous air, but wants a sufficient quantity of *latent heat*, which is imparted to it in these processes. Perhaps, also, it may be found, that fixed air, the acid, and alkaline airs, or any others that are readily imbibed by water, contain less latent heat than those kinds of air which are less affected by water. Very little heat is produced by the union of acid or alkaline air and water, though, as I have found
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by experiment, there is *some* produced, whereas the decomposition of dephlogistigated and inflammable air never fails to produce a very great degree of heat.

The phenomena attending the accension of pyrophorus throw some difficulty into the theory relating to the part that the *air* has to do in it. For by its firing equally well in dephlogistigated and nitrous air, it should seem that there is something in common to them both, and that the dephlogistigated nitrous air (in which it will not take fire) has not this common ingredient; which, however, the preceding theory supposes. The firing of pyrophorus probably arises from the sudden absorption of any kind of air, or water, which then gives out its latent heat (for there can be no doubt that every fluid in the form of air so far resembles steam, as to contain latent heat). But the accension of it should seem to be effected by the attraction that the pyrophorus has to some one principle on which the elastic form of the air depends; and, according to the preceding theory, there is nothing but the

dephlogisticated nitrous vapour that equally enters into the composition of *dephlogisticated* and of *nitrous air*, and that is likewise found in *dephlogisticated nitrous air*, in which it will not take fire. Perhaps, therefore, this accention may depend on the *mode of combination* between the principle of latent heat, and other principles in the constitution of these kinds of air, and of this we are entirely ignorant.

The action of *electricity* on the different kinds of air is very little known. In some cases it seems to communicate *phlogiston*, but more generally mere *latent heat*. It is however, a subject deserving of the most particular attention, and it seems to be not far removed from our reach.

After these theoretical speculations, in which I fear I have not communicated much light, though it is as much as I have been able to get, I proceed to make some observations relating to *phlogiston*, the existence of which is at present a great subject of discussion with philosophers; some maintaining that there is no such thing, and

others holding the doctrine of STAHL on the subject.

The term *principle* may be applied to any cause of a known effect, whether it be what logicians call a *substance*, or a *property*. Thus, while there is in nature such a thing as *heat*, it must also be said, that there exists a *principle*, or *cause of heat*, whether it consist in the mere change of state in the parts of the heated body (as, for instance, in their being thrown into a vibratory motion) or there be such a *substance* (in the same sense in which air, or water, are substances) infused into the body when it is heated, and withdrawn again when it is cold.

According to STAHL, phlogiston is a real substance, capable of being transferred from one body to another; its presence or absence making a remarkable difference in the properties of bodies, whether it add to their *weight*, or not. Thus he concluded, that oil of vitriol, deprived of water, and united to phlogiston, becomes sulphur; and that the calces of metals, by the addition of the
same

same substance, become metals. The *air* that has since been discovered in the calces of metals, makes no great difference in the system. For as oil of vitriol must part with its water, as well as imbibe phlogiston, in order to its becoming sulphur; so the calx must part with its air, as well as imbibe phlogiston, in order to become a metal.

What is now contended for is, that in the oil of vitriol changing into sulphur, something is lost, and nothing gained, and also that a calx becomes a metal by the loss of air only. And did facts correspond to this theory, it would certainly be preferable to that of STAHL, as being more simple; there being one principle less to take into our account, in explaining the changes of bodies. But I do not know of any case in which phlogiston has been supposed to enter into a body, but there is room to suppose, that something does enter into it; and in other cases, and especially some of my own late experiments, something certainly does. At least, the hypothesis that has been proposed in its place, concerning

the constitution of those bodies, which had been said to contain phlogiston, is clearly overturned by them.

What has been insisted upon, as most favourable to the exclusion of phlogiston, is the revival of mercury, without the addition of any other substance, from the precipitate *per se*. In this case it is evident, that mere heat, either in a close retort, or in *vacuo*, is sufficient to revive the metal. And as what is expelled from this calx is the purest dephlogisticated air, it has been said, that mercury is changed into this calx by imbibing pure air, and therefore becomes a metal again, merely in consequence of parting with that air.

But Mr. KIRWAN explains this case in the following manner, which to me appears perfectly satisfactory. The metal, when exposed to a certain degree of heat, in contact with pure air, or with any thing that contains pure air (as in that very fine experiment of the Count de Morozzo with fixed air) imbibes, indeed, the pure air, and nothing else; but the phlogiston belonging
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to the metal unites with that air, so as together to form *fixed air*; and therefore the calx may be said to be the metal united to fixed air. Then, in a greater degree of heat than that in which the union was formed, this factitious fixed air is again decomposed; the phlogiston in it reviving the metal, while the pure air is set loose. Consequently the precipitate actually contains within itself all the phlogiston that is necessary to the revival of the mercury*. Since, therefore, this fact *can* be accounted for without excluding phlogiston, the supposition of which is exceedingly convenient, if not absolutely necessary, to the explanation of many other facts in chemistry, it is at least adviseable not to abandon it.

Besides, as it is known that *light* contains phlogiston, if there be any such thing, and

* Since it is known how much dephlogisticated air is got from a given quantity of precipitate, and how much phlogiston this dephlogisticated air requires in order to compose fixed air, we may, from these *data*, easily ascertain how much phlogiston enters into the composition of mercury.

there can be no *red heat* without light; it may be said, that all the phlogiston that is necessary to the revival of this particular metal may enter into the heated calx, notwithstanding all our endeavours to exclude it. But it does not yet appear that light can impart so much phlogiston, as sensibly to add to the weight of any body; and therefore it can hardly be supposed to impart so much as is necessary to the revival of a metal, the phlogiston in which is probably half the weight of the inflammable air with which it is revived.

But the experiment which, in my opinion, proves decisively that the principle which has hitherto been called phlogiston is a real *substance*, and even adds considerably to the *weight* of bodies, is that of the decomposition of charcoal by the heat of a burning lens in vacuo, or by means of steam in a hot earthen tube. According to Mr. LAVOISIER, charcoal is a simple substance, capable of decomposing the water that comes into contact with it, but contributing nothing to the inflammable air that is procured

cured in the process. But here we see almost the whole of the charcoal disappear, at the same time that nothing but inflammable air (though not of the most simple kind) is produced. Nothing is found of the charcoal, besides a very inconsiderable portion of *white ashes*, which could not possibly have imbibed the dephlogisticated air belonging to the water, and by that means have set the inflammable air at liberty; which might be said with respect to the similar experiment with iron.

It may be said, that the dephlogisticated air from the water goes to form the *fixed air* that is likewise produced in this process, together with the inflammable air. But I have observed, that when no more water is used than what is absolutely necessary for the purpose (as in my experiment with charcoal in *vacuo*) there is no fixed air mixed with the inflammable. What then becomes of the substance of the charcoal, if it does not supply something which may be called phlogiston from which the inflammable air is made? The air certainly ex-

ceeds the weight of the charcoal that is destroyed, but the substance of the charcoal being found no where else, must be in the air, as a constituent part of it.

It will be said, that the air that is formed by heating charcoal, and combining it with steam, is not mere water and phlogiston, because, when it is decomposed together with dephlogisticated air, a considerable quantity of *fixed air* is produced. But as fixed air appears from the latest discoveries not to be a simple substance, but to consist of pure air, and something else that answers to the definition of phlogiston, this principle in that fixed air must have been furnished by something in the charcoal. And as almost the whole of the charcoal goes into the air that is produced, it must necessarily make part of its *weight*, how difficult soever it may be to ascertain the exact amount of that weight.

Whatever may be objected to this reasoning, the experiment with charcoal and the scales of iron, which together yield abundance of inflammable air, is, as I have ob-

served,

served, a clear refutation of Mr. LAVOISIER's hypothesis, which was thought to overturn the doctrine of phlogiston. For he supposes, that neither of these substances contain any thing that can constitute inflammable air.

○ If this hypothesis of Mr. LAVOISIER cannot explain the present known facts, and the doctrine of phlogiston *can* do it, the latter ought as yet to be retained, in preference to his, or any other that has yet been proposed.

That calces do not become metals merely by parting with the air they contain, is evident from my experiments on heating them in contact with inflammable air, in which the inflammable air, or some necessary part of it, is undoubtedly absorbed; and though a little moisture be deposited in the process, it may well be supposed to be that which, in conjunction with phlogiston, constituted the inflammable air. And what can the other principle that is absorbed by the calx be, but the same thing, which, when united to water, is recovered again from

from the metal, and found to be inflammable air, having all the same properties with that which was employed in the revival of it. Metals, therefore, are not simple substances, but consist of their calces, and something else, which they take from inflammable air. And as the same may also be taken from any combustible substance, it corresponds exactly to STAHL's phlogiston, and therefore the doctrine of it is confirmed by these experiments; that is, we must still say, that, in all combustible substances, there is a principle capable of being transferred to other substances, which, when united to the calces of metals, makes them to be metals, and which, united to oil of vitriol (deprived of its water) makes it to be sulphur.

S E C-

SECTION XXIX.

Experiments made since the Sections in this Work to which they relate were printed off.

1. **B**EING desirous of ascertaining, as perfectly as I could, the nature of that *black matter* which I procured by decomposing dephlogisticated and inflammable air, I frequently exploded great quantities of the two kinds, and never failed to collect it! But I have not yet been able perfectly to satisfy myself with respect to the nature of it. From the following experiments it seems pretty certain, that *mercury* is necessary to its production, and I have never found it in vessels in which mercury had not been contained. I did not, however, find it to be acted upon by spirit of nitre so readily as that *black powder* which is got by agitating mercury in pure water (which can be nothing but mercury superphlogisticated)

phlogisticated) but thought that it approached nearer to the action of that acid upon *foot*, the blackness of which is not discharged except by frequent affusions of that acid.

In a former volume I have observed, that the black powder, procured by agitating mercury in water, becomes white running mercury the moment that it is dry; but I have lately found some exceptions to this observation. Having a large quantity of this black matter in a glass phial, it happened to be broken by the freezing of the water with which it was mixed. I did not notice it at the time, but I afterwards found a considerable quantity of running mercury in the place where the bottle had stood, and besides that a thick cake of dry black matter, which however appeared to be nothing but mercury, as it was completely dissolved in spirit of nitre.

I must observe farther, that some of this black matter which I have lately procured, by agitating mercury in water, has not always become perfect running mercury by the

the evaporation of the water with which it was mixed, but has sometimes left a black stain upon the vessel in which it was contained. This, however, has disappeared by the affusion of spirit of nitre. I shall now relate some particulars of the processes by which I procured the black matter from the decomposition of inflammable and dephlogisticated air.

When there was much water in the vessel in which the explosions were made, there was no black matter produced, though the lower part of the vessel contained mercury; and though by repeated explosions in these circumstances the water did become turbid, and deposit a black sediment, it was by no means so much as I always got when I only just moistened the inside of the vessel, by filling it with mercury immediately after there had been water in it. In this last case, all the inside of the vessel never failed to be made exceedingly black by every explosion; and repeating this process, I was able, by pouring in water, to collect a considerable quantity of black sediment. From
this

this it should seem, that the inside of the vessel contained a subtle invisible *vapour of mercury*, which became black by the phlogiston from the decomposed inflammable air; and this black powder, as I have observed, is not much affected by spirit of nitre, but has very much the property of *foot*, in that respect, which is a circumstance that certainly deserves to be farther investigated.

2. I have repeated with particular care the experiment of decomposing nitrous air by the electric spark; from which it appears that, in this process, though not in that of heating iron in it by the sun beams, some *acid* is really deposited from the air. But it is so little, that it may perhaps be supposed to have been *contained* in it, as an extraneous substance, not separable from it by standing in water. It is also to be observed that, in this process, I was never able to produce any dephlogisticated nitrous air. Whenever the experiment was suspended, the air was always found to be either nitrous or phlogisticated.

In

In one experiment of this kind, to which I gave particular attention, an ounce measure and a half of nitrous air was reduced to 0.4 of a measure by electric explosions, taken in a vessel of mercury, with a little pure water on the surface of it; an iron wire introduced through the mercury, and into the air, being used for the purpose of taking the explosion at a proper distance. After the greatest possible diminution of the air I could perceive no change of colour in the water, but after a few hours standing it had a purplish colour, and after a day and night, it was become of a deep red, and very turbid, evidently from the solution of the iron. The water was acid to the taste, but in the slightest degree imaginable; and I could perceive by the smell of it, that there was a little nitrous air still contained in it, though I could not perceive any redness on exposing the vessel to the open air. A lighted candle being dipped into it, was immediately extinguished.

There was one circumstance which I cannot easily account for in this process. After

ter the diminution of the air would proceed no farther, I observed that, after some time, there was an increase of the quantity of it, viz. about one-tenth of an ounce measure; but after this the air was gradually contracted to its former dimensions, without any farther use of electricity.

I repeated this experiment with the same quantity of nitrous air, and it was diminished in about the same proportion as in the preceding. But examining the water before it had been discoloured by the solution of the iron, I could only perceive some astringency in the taste of it, though it reddened a considerable quantity of water tinged blue with the juice of turnsole. There was also still something nitrous in the residuum of this air. For being mixed in equal quantities with common air, the standard was about 1.8. It contained no dephlogisticated nitrous air, as it was not sensibly diminished by agitation in water.

As the electric spark cannot be supposed to communicate so much phlogiston, as the heating of iron by the sun beams, we may be

be better able by this process to ascertain the quantity of phlogiston contained in phlogisticated air; since the residuum probably contains all the phlogiston belonging to the nitrous air from which it is made, and if we may calculate from the number in the former of these experiments, phlogisticated air must contain about four times as much phlogiston as an equal bulk of nitrous air; or, as appears by my former experiments, four times as much as inflammable air made from iron by oil of vitriol, or steam, of which one half is probably phlogiston, and the other half water.

3. Having heated *iron* in nitrous air, I proceeded to heat in the same air a piece of *charcoal*, not long after it had been subjected to a strong heat covered with sand. The sun not shining immediately after the charcoal was introduced into the vessel of air, (through the mercury, by which it was confined) part of the air was absorbed; but on heating the charcoal the quantity was soon increased. Having continued the process as long as I thought necessary, I examined

the air, and found it to be about as much as the original quantity of nitrous air, but it was all phlogisticated air, extinguishing a candle, and having no mixture of fixed air in it. At one time, when I dipped a lighted candle into it, I thought there was an appearance of its containing something inflammable, but it was in the slightest degree imaginable. There was, however, evidently something inflammable in the air expelled by water from this charcoal afterwards.

When I heated *charcoal of copper* in nitrous air, there was a small addition to it of perhaps a twentieth part; and being examined, it was found to contain two-sevenths of its bulk of fixed air, and the rest was phlogisticated. This change in four ounce measures of the air was produced by the loss of between one-fourth and one-half of a grain of the charcoal, and the copper was evidently revived.

That fixed air should be produced in this experiment, and not in the preceding, is rather extraordinary. In this circumstance it resembles

resembles the experiment on the heating charcoal of copper in dephlogisticated air.

4. It is something remarkable that heating the *scales of iron* in nitrous air, has the same effect as heating iron itself in it, but then much more *time* is requisite to produce the same effect.

I heated a quantity of scales of iron in nitrous air till 2.7 ounce measures were reduced to 1.35, and then found it to be wholly phlogisticated air, extinguishing a candle, and neither affecting common air, nor being affected by fresh nitrous air. During the process the surface of the scales on which the focus of the lens fell were affected in a very peculiar manner, as if they were superficially melted; the parts being in a state resembling that of ebullition, which continued a long time on the same spot. No moisture was produced in the process, as when these scales are heated in inflammable air.

When I put the scales which had been used in this experiment into diluted oil of vitriol, it became white like milk, and very

little air was produced, even by means of heat. I got, however, a few bubbles; so that with good management I could just be sure that it was not fixed air, that it did not affect nitrous air, nor was affected by it.

5. I treated in the same manner some iron that had been used to diminish nitrous air by the heat of the sun. But though I diluted the oil of vitriol with only two equal parts of water, it was but weakly acted upon, and much heat was necessary to effect the solution of it. During the process much *black matter* was formed, and the inflammable air produced had an offensive smell. In these respects this iron seemed to approach to the nature of *cast iron*; but when it was melted by a burning lens in the open air, the parts of it were not dispersed like those of cast iron.

6. I took the electric spark in a quantity of *dephlogisticated nitrous air*, till, without any perceivable change in its quantity, it was become immiscible with water. Then, admitting distilled water to it, there was no
I deposit

deposit made, though I could perceive a white incrustation quite round the inside of the glass vessel, contiguous to the mercury by which the air was confined. But admitting to it the water in my trough, which happened to have mixed with it various solutions of iron, there was a copious white precipitate. This substance I had got once before, and found that by exposure to a red heat it became of a brown colour, and adhered firmly to the pieces of earthen ware on which it was heated, thereby appearing to be ochre. The whiteness may have been occasioned by the mercury.

These experiments on nitrous air, and the several modifications of it, especially those in which iron, and other substances, are heated in them, and examined afterwards, seem to promise much more light on the subject, and on the doctrine of air in general. But I am obliged to leave them in the present unfinished state, in which I do not chuse to risk any inferences from them. When I shall have good sun-shine, and especially if I should succeed in procuring a more

powerful burning lens, I shall eagerly resume the experiments, and my friends, and probably the public, will soon be informed of the result of them.

To the Account of these Experiments I shall add an Illustration of the Note, p. 103.

THE water expended was 336 grains, and the iron gained 267 grains. Supposing it to have lost phlogiston equal to half the weight of the inflammable air, viz. 840 ounce measures = 25 grains (the whole weight of that air being 50 grains) the water that really entered into the iron must be estimated at 292 grains (which is $367 + 25$). This deducted from 336, leaves a remainder of 34, which is not much more than 25, or half the weight of the inflammable air.

THE

APPENDIX.

NUMBER I.

*A Letter from JAMES KEIR, ESQ. F.R.S. relating
to a Metallic Pyrophorus.*

DEAR SIR,

WHEN I last had the pleasure of seeing you,
I happened to mention a new kind of
metallic *pyrophorus*, that had occurred to me, the
accension of which was accompanied with a pleas-
ing phenomenon, namely, the instantaneous
revival of the metal which entered into its
composition. You expressed a desire of being
informed more particularly of the attending cir-
cumstances, which I am happy to comply with,
although I doubt whether the matter is worth
your notice—but this I submit to you.

I filled about five-sixths of the contents of a copper cylindrical box, which had a lid fitted to it, and which was three inches in diameter, and two inches in depth, with saw-dust, which I pressed down; and I laid upon the saw-dust, as much well-washed *plumbum corneum*, as entirely filled the box, which I then covered with its lid. I placed the box on the coals of a chamber fire, so that its bottom only should be in contact with the fuel, and I kept it on the fire, till no more vapour seemed to issue at the joining of the lid. I then removed it from the fire, and while it was hot, I closed up the joining of the lid with sealing wax, by which means the external air was excluded. After it had stood in the cold about ten hours, I opened the box; and the corneous lead, which was very white before the operation, was now rendered black by the vapour which had arisen from the saw-dust, and which was obliged to pass through the lead before it could escape. This black metallic mass was no sooner exposed to the air, than ignited sparks appeared, which spread more and more, while the lead was seen to revive in the form of minute globules, and the part which did not revive, was changed into a yellow powder, or calx of lead. It is to be observed, that before I opened the box, I placed it at the side of the fire, in order to melt the
sealing-

sealing-wax, to enable me to separate the lid. It is possible, that this small degree of heat may be necessary, or conducive, to the accension. I ought also to acquaint you, that the preparation of this pyrophorus, requires nicer attentions than that of any which I am acquainted with. For a small excess of heat will revive the lead, which will spoil the experiment. Also, if any air be admitted through the joints of the vessels employed, the kindling property will be prevented by the absorption of the air, which in this case is generally too gradual to produce inflammation. The metallic substance in this state of impregnation with inflammable matter, although not a pyrophorus, is an exceedingly quick tinder. For when touched, however slightly, by an ignited body, it will instantly kindle, and the fire will spread over the whole piece, reviving the lead wherever it goes, and exhibiting a very beautiful example of metallic reduction, not unlike the familiar experiment of reviving the lead of a wafer containing minium at the flame of a candle; but with this difference, that the fire in the wafer requires to be kept up by flame, whereas in this metallic tinder, it spreads, and creeps spontaneously along without flame, over the mass.

In

In the present unsettled state of chemical theory respecting *combustion*, I will not encroach on your patience, by attempting to explain the mode of action of this and other pyrophori; although it requires but little ingenuity to frame hypotheses, with the advantages which philosophers now frequently indulge themselves in, of making a free use of various supposed principles, as *phlogiston*, *matter of heat*, *ferbile principle*, *humor*, &c. of the existence of which we have unfortunately none of the proofs, that in other instances are thought requisite to evince the presence of matter. I shall, nevertheless, make one or two remarks, which perhaps may eventually tend towards a future explanation.

As in all known instances of pyrophori (which word I confine to such substances as having been ignited and afterwards cooled, retain the power of kindling in a degree of heat much less than that of ignition, upon exposure to air) one essential ingredient is the inflammable matter of animals, or vegetables; for no pyrophorus has yet been made with sulphur, without the addition of other inflammable matter: and as the coal of animal and vegetable substances properly treated, without addition of other matters, has been observed to possess a pyrophorous quality; there seems to be some reason to suspect, that those
adventitious

adventitious substances which have been employed in the preparation of pyrophori, do only act as accessaries, by placing the animal or vegetable matter in proper circumstances for accension. Thus it is an established fact, that moisture facilitates the accension of HOMBERG's pyrophorus, and I think it is probable, that the use of all the ingredients, excepting the flour or sugar, in that class of pyrophori, is to form a liver of sulphur, which by its powerful attraction of moisture, produces a degree of heat which enables the pyrophorous vegetable matter to kindle. For we know that a moderate heat will often effect the accension of weak pyrophori, which without this help would not kindle, and Mr. BEWLEY has shewn, that if instead of moisture, heat be directly applied, the effect will be produced. That moisture is necessary in the ordinary heat of the atmosphere has been proved by Mr. SCHEELÉ, who dried the air of a vessel containing pyrophorus so effectually, by means of quick-lime, that the pyrophorus remained inactive. The error of Messrs. SAVIGNY and MACQUER, in supposing that the attraction of moisture was caused by a disengaged concentrated acid has been fully shewn by several chemists; for no such acid exists in the pyrophorus, but on the contrary, there is a very dry liver of sulphur, which

which is equally attractive of moisture, and equally at least capable of producing heat by the union. For besides the heat produced directly in consequence of the chemical union of liver of sulphur with water, an additional heat arises from another cause, viz. from the change of the state of the water, or its transition from fluidity to solidity, which takes place in this instance. For a liver of sulphur of this kind will imbibe a considerable portion of water, and remain still dry.

But although the attraction of moisture may be considered as facilitating the operation of the pyrophorus, I cannot agree with Messrs. SAVIGNY and MACQUER in considering it as the sole, or principal cause of the accension. For, as we know from experiment, that pyrophori will not kindle in any but pure air, or fluid containing it; and farther, that this pure air is actually absorbed by them; we seem to have reason to believe, that the absorption of air by the vegetable or animal inflammable matter is the immediate cause of accension; as indeed it seems to be of all other kinds of combustion. We may be warranted, I believe, in making one step farther towards the explanation of this phenomenon, by referring the cause of the production of heat in this instance of absorption of air, to the general fact or law, which seems to be now pretty well established,

established, namely, that heat is produced in all cases, in which bodies undergo a change of condition, whereby they are made to occupy less space than before, and especially when they pass from a fluid to a concrete state.

In what manner the *plumbum corneum* in the pyrophorus, I have described, contributes to facilitate the accension of the inflammable matter of the saw-dust, which impregnates the lead, I know not, unless by furnishing to this very fine charcoal, or condensed vapour of the saw-dust, a proper receptacle which detains it, and gives it a very enlarged surface, by which the air may be very copiously applied. I have endeavoured to make a similar pyrophorus by treating minium, and cerufs, in the same manner, but by no means with equal success. The difference may perhaps be owing to the much greater solidity of the last mentioned preparations of lead, than of the *plumbum corneum*. Consequently they are less capable of giving an extended surface, on which the inflammable vapour of the saw-dust might deposite itself.

I am, with respect and esteem,

Dear Sir,

Your most faithful Friend,

and most obedient Servant,

WINSON-GREEN,
MARCH 31, 1786.

JAMES KEIR.

N U M B E R II.

A Letter from Mr. THOMAS HENRY, F.R.S. concerning the Phlogiston of Alkali.

DEAR SIR,

Manchester, March 22, 1786.

I RECEIVED your obliging messages, through the hands of ————. I do not recollect any thing that has occurred to me, worthy of communication, except you should deem the following experiment, and the inferences drawn from it, of sufficient importance. But should they appear to you unworthy of public notice, I rely on your friendship to suppress them.

The discovery of the identity of phlogiston and inflammable gas; or, that inflammable gas is *little else* besides phlogiston, suggested to me the thought that, probably, phlogisticated alkali might be readily prepared, by throwing streams of that gas into a solution of caustic fixed alkali. I accordingly filled the middle part of a small Nooth's machine, the capacity of which was about half a pint, with such a solution; and continued, for some days, to pass a current of inflammable gas, from iron filings and vitriolic acid, contained in the lower part of the machine through

through the liquor. The solution acquired a brownish cast, and considerable quantities of gas, confined between the middle and highest compartments of the machine, disappeared, *though I will not say they were absorbed*; notwithstanding the caustic and alkaline taste of the solution was not impaired, nor did it acquire the property of precipitating Prussian blue, from a solution of martial vitriol. The vessel being left in a window, after I supposed the discharge of gas had been finished, the sun's rays falling on it, produced a fresh separation, and so rarified it, that the vessel burst with a loud explosion. I was, therefore prevented from examining whether the solution had really absorbed any considerable portion of the gas.

I was at that time unacquainted with the discovery made by Mr. SCHEELÉ and others, that the matter which is united to fixed alkali, during its calcination with blood, is not pure phlogiston, but a peculiar acid. Yet I considered the above experiment as affording strong cause for suspicion, that mere phlogiston is not capable of neutralising, or rendering mild, alkaline salts; and as affording a probable answer to the arguments that have been used by Mr. DELAVAL, in his excellent paper *on the cause of the permanent colour*

*colour of opaque bodies**, to shew that the power, which fixed air possesses of reducing alkaline salts to a mild state, depends, not on any acid quality, but on the phlogiston which forms one of its constituent parts. For were this opinion well founded, it might reasonably be expected that inflammable gas, which contains so much larger a proportion of phlogiston than is combined in fixed air, should be, at least, equally capable of destroying the causticity of alkalis.

I am, with sincere esteem,

Dear Sir,

Your faithful and obliged

Friend, and Servant,

THOMAS HENRY.

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